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CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

ON THE NATURE OF MATHEMATICAL AND SCIENTIFIC DEMONSTRATION[1]

IN the development of every science there is a growth of method as well as of results. We are accustomed to give close attention to the latter, and frequently we reorganize them into connected and logical wholes so that every student may conveniently view them in their entirety and in their proper relations to one another. In determining the method by which the matter shall thus be organized we are generally guided by considerations of convenience in exposition.

In much of our teaching, likewise, the selection and arrangement of material is determined primarily by a desire to arrive at results in the most expeditious manner possible.

One effect of this controlling emphasis, both in lecturing and in the writing of books, is that many of us never come to a proper appreciation of the labor which has been expended in perfecting our tools of investigation and never have a vital conception of the character of the important problem of method. Such a person usually will be able to employ only the tools which are presented to him by others. He will not be able to devise a new method to meet the needs of the new problem which arises in his own work.

Now the most important steps forward are made by the introduction of new methods of advancement. It is obvious that the person most likely to discover the

[1] An address delivered on the evening of October 6, 1913, to "The Euclidean Circle," an organization among the graduate and undergraduate students of mathematics in Indiana University.

new method is the one who understands best the fundamental ideas on which the methods of his subject are based and the relation of these ideas and methods to corresponding ones in allied fields of study.

It is, therefore, important to the student of every science to analyze the growth of method in his science and to ascertain the fundamental basis on which it has developed. This analysis requires a wider grasp of the subject than the student can possess in the early years of his labor. But he can appreciate, to a large extent, the results of such an analysis and profit by a knowledge of them, if they are presented by some one of a fuller experience than himself.

It is my purpose this evening to present to you the outcome of such an analysis of the nature of mathematical and of scientific demonstration.

A method which was considered useful and legitimate in one generation has often been discarded in the next. Sometimes it has been replaced by another which was merely more powerful and at least equally convenient. At other times it has been found to be not a legitimate method; and it has been necessary to abandon it because investigators could no longer be sure of results obtained by means of it. This has been true both of mathematics and of experimental science, but less frequently of the former than of the latter.

For a mathematical method a first requisite is that the mind shall assert with the strongest emphasis that the method is legitimate. We shall say nothing about how this conviction may have arisen: we shall first demand of it only that it shall be a profound and universal conviction of the human mind.

I shall illustrate what I mean here by an example. Let us take the principle or method of mathematical induction. It is convenient to consider a particular case of its use. Suppose that we wish to demonstrate the binomial theorem,

$$(a+b)^n = a^n + na^{n-1}b + \cdots + nab^{n-1} + b^n,$$

for every positive integer exponent n. Our method of procedure is as follows: We first observe that the theorem is true for n equal to 1. The next step is to prove that if it is true for n equal to k, where k is any positive integer, it is likewise true for n equal to $k+1$; and we shall suppose now that this step has been made by the necessary argumentation. Now we know that the theorem is true for n equal to 1; from the result last mentioned we conclude further that the theorem is true for n equal to 2. Since it is true for n equal to 2 we may apply our previous result again and conclude that is is true for n equal to 3. Likewise we proceed to the case when n is equal to 4; and so on.

Now, if one analyzes the principle on which this argument is based, the conclusion comes home to him with a compelling force; and he can not fail to have confidence in it. He has verified the theorem perhaps in only a few cases; but he has no fear that a case will ever be found to contradict it.

The first requirement of a mathematical method, as I have said, is that it shall possess just this property of compelling confidence in the conclusions reached by its means. The ground of this compelling power in the method the mathematician (as such) does not seek to find; that is a problem for the philosophers.

But such credentials as those mentioned, however good they may appear to be, are never accepted by the mathematician as entirely satisfactory. He does not, indeed, dispute their legitimacy. But, through much experience, he has found that methods exist concerning which the uninitiated

mind asserts emphatically that they are valid, whereas he knows cases in which they lead to inconsistent results.

Therefore these credentials are treated by the mathematician as affording him only a means of making a first choice of methods to be examined. They are still to be subjected to tests in the laboratory of the mind.

You may ask: To what sort of test may one conceivably subject a method which the mind approves with as much confidence as it does that of mathematical induction, for instance? There seems to be just one such test available. Does it always lead to consistent results? I do not say true results; for there is no one to determine whether the results are true. If several methods are involved at once, it is to be demanded of them also that the results obtained by means of any of them shall be consistent with those obtained from others.

Effectively, what the mathematician does, then, is to select a number of methods in the intuitional way which I have indicated and then to subject them to the most exacting requirements in the way of consistency of results obtained by their use—results exact in their nature and deduced from exact data and covering a wide range of thought.

The only methods which he retains after these extended tests are those which have never been known to lead to a contradiction at any time in the history of human thought. One other analysis must finally be made before they can be admitted into the privileged circle of mathematical methods. It must be ascertained of a given method whether it is perfectly precise in its nature in the sense that no two persons of intelligence have a different opinion as to what the method is. There is no disagreement, for instance, among thinkers concerning the definition of mathematical induction.

Once the mathematician has selected some methods which he is willing to employ, he uses them in argument in the coldest and most formal way. In making discoveries intuition plays a most important rôle and is a precious guide which he can not dispense with. But when he states his proofs he does it in terms which are entirely free from intuition. Further, he is careful to make sure that he has used no methods except those which have already successfully passed his most searching scrutiny. Through sore experience he has learned that safety lies in no other direction.

But this is not all. Every new use of his methods gives rise to the possibility at least that a contradiction has crept in through some argument which has never before led into such error; and this possibility must be examined—certainly in all cases where the research opens up a new field of thought, if not also in the more common investigations.

It is due to this extreme carefulness on the part of the mathematician that we have so strong a feeling of certainty in his conclusions. But if we analyze this feeling with care we shall find, unexpectedly perhaps to most of us, that it is due after all to our experience with the methods employed, since under the most severe tests they have never led us into contradiction. (They are the only methods which possess this latter property.)

If you will recall what I said about the way in which the mathematician has selected his tools of investigation, you will see why he can never be absolutely sure that he has employed a proper procedure in argument. At no stage in the development of his method was there an absolute criterion according to which a method was to be

retained. He proceeded entirely by exclusion. First, all conceivable methods which did not come up to a certain standard were put aside. Those that remained were subjected to further tests, one after another, and some of them were found to be unsatisfactory. Those left over were finally retained because they had the negative recommendation of never having been caught in an act of deception.

What shall we say then of the certainty of mathematical doctrine at the present day? To answer this question, let us observe that, in all preceding generations, methods in mathematics have been used with confidence which, in the experience of a later day, were found to be not legitimate; they have been discarded, sometimes after generations of confident use. It is not likely that men have heretofore always made mistakes of this kind and that we have suddenly come upon an age in which mathematical methods are certain in the absolute sense.

We are then forced to the conclusion, however unwelcome it may be, that the certainty of mathematics is after all not absolute, but is relative. To be sure, it is the most profound certainty which the mind has been able to achieve in any of its processes; but it is not absolute. The mathematician starts from exact data; he reasons by methods which have never been known to lead to error; and his conclusions are necessary in the sense, and only in the sense, that no one now living can point to a flaw in the processes by which he has derived them.

When we find ourselves forced to this result, our first feeling is probably one of disappointment. But a deeper analysis of the matter will bring us to a different attitude. It gives us a new sense of the problem which lies before us in the development of mathematical thought. We have not merely to seek new results; but we have also the larger problem of method to inspire our activity and to lead us perhaps to fundamental achievement.

It is conceivable that methods may be devised by means of which we shall attain to well-nigh perfect certainty. Let us suppose that we have found a method of argument, or a principle A, which has this property, namely: In whatever way we start from a principle not in accord with it we shall be led into results which are themselves mutually contradictory. Now suppose that principle A is itself not a legitimate one. Then there is a legitimate principle B not in accord with it. From this new principle we can get mutually contradictory results. That is, principle B is both legitimate and not legitimate. This being a contradiction in itself, we conclude that the hypothesis from which it is deduced is false. Therefore principle A is legitimate. I say that it is conceivable that such principles A will some day be discovered; but they have not yet been found.

In an earlier day, and of course without the aid of such principles as I have just mentioned, men apparently had come to a feeling of absolute certainty about the accuracy of mathematical conclusions. Those fundamental methods of argumentation, of which I spoke in the outset, they conceived to belong to a class of innate or inherent ideas which had been put in the mind of man by the Creator. The initial hypotheses and basic notions of a mathematical discipline they thought of as belonging to the same category. If these innate ideas did not have all the elements of absolute certainty, there could be only one conclusion: the Creator had deliberately deceived man. Since they considered this to be absolutely impossible, they had complete confidence in the certainty of mathematical results.

This is merely one example of the usual dependence of the ancients on the authority of abstract reason. By this means they sought absolute certainty in scientific as well as in mathematical and philosophical thought. A brief account of their general point of view in regard to this matter will serve to connect the two topics which I have asked you to associate together this evening; for it is in the ancient time that the two methods are most closely related.

It is convenient to speak of the position of Plato. This philosopher refers, with a touch of contempt, to one who gives his life to the investigation of nature, feeling that such a person was concerned with the visible universe alone and was immersed in its phenomena. These, whether past or present or to come, admit of no stability and therefore of no certainty. "These things," he says, "have no absolute first principle and can never be the objects of reason and pure science." Plato believed that the senses are deceptive and could never lead to the discovery of truth. The only way to develop science was to look within and find there the fundamental principles on which it should be based; and then to develop logically the consequences of these principles.

But I shall not take up your time with an analysis of these old opinions, however much they may have influenced or retarded science in times past. Neither shall I pause to indicate how the old Greek science, such as it was, came into a place of authority, dominating the thought of many generations and giving rise to a fearful intellectual stagnation. I prefer to come to the time when the development of scientific method began to recover men from their stupor and to kindle a new intellectual light and fervor.

Let me direct your attention to the Italian philosopher Bernardino Telesio (1509–1588) as the great figure who marks the period of transition from authority and reason to experiment and individual responsibility. He was the forerunner of all subsequent empiricism, scientific and philosophical, sowing the seeds from which sprang the scientific methods of Campanello and Bruno, of Francis Bacon and Descartes and the scientists of our day. He abandoned completely the purely intellectual sphere of the ancient Greeks and other thinkers prior to his time and proposed an inquiry into the data given by the senses. He held that from these data all true knowledge really comes.

The work of Telesio, therefore, marks the fundamental revolution in scientific thought by which we pass over from the ancient to the modern methods. He was successful in showing that from Aristotle the appeal lay to nature; and he made possible the day when men would no longer treat the *ipse dixit* of the Stagirite philosopher as the final authority in matters of science.

It is true that Telesio had been preceded almost three centuries by Roger Bacon (1214?–1294?), a modern thinker in the middle ages, whose conceptions of science were more just and clear than those at a date four centuries after his birth. But this Bacon was a man born out of time, too far in advance of his age to be appreciated by it; and consequently he had but little influence on the growth of scientific method. The balance has now been restored in his favor, so far as the judgment of historians is concerned; but that leaves untouched the facts of effective scientific progress.

Telesio had several followers, or perhaps we should say fellow pioneers, in the same field. Among these Francis Bacon probably stands out as the most prominent of all. He said of himself that he "rang the bell which called the wits together." But his contributions to the stock of actual scien-

tific knowledge were practically inconsiderable. His great merit lay in his making men see that science was in fundamental need of a new method. The method he suggested was not adopted; but his analysis of the need was the signal for the search which has ended in modern science.

I need not take you further through the long history. It is sufficient to my purpose to point out that primitive man first developed by experience a way of his own for observing and fixing in mind external phenomena, that the Greeks seized upon their own and their predecessors' observations and sublimed experience into theory, that Telesio and Bacon and others taught mankind the insufficiency of Greek methods and the need of new ones, and that modern science came into being and fulness of stature through generations of workers who sought to put, and succeeded in putting, the new ideas into the form of effective tools of advancement.

From this brief historical account it is seen that the method of experimental science has itself grown through experiment. The style of argument employed by Plato, for instance, has been entirely superseded by another and better. Man had to learn by the experience of failure how to ascertain the true relations of phenomena. In other words, there was no "preestablished harmony" between the mind and the phenomena it had to interpret of such character as to lead the former to a ready explanation of the latter.

Our progres in this respect has been over a hard and long and rough road. We go a very short distance, relatively, into our past to find the time when methods were uniformly employed in science which are now known to be quite untrustworthy. What is the bearing of this fact on our confidence in the conclusions of science? In order to answer this question properly we shall have to analyze briefly the general nature of scientific investigation as at present practised.

In the first place, scientific demonstration starts from data which involve the ever-present inexactness which is due to experimental error. In the nature of things it is impossible that the argumentation should ever have an exact basis to rest upon; and consequently all conclusions must again be tested by a direct appeal to phenomena. In another important respect also the method is essentially different from that employed in mathematics. Here intuition is a fundamental guide in argument as well as in discovery; and a "proof" whose leading elements are grounded in intuition is accepted with a confidence at least equal to that which is accorded to one characterized by mathematical precision and rigor.

One result of this inexact basis and especially of this loose method of argumentation is that the conclusions reached often are primarily of the nature of inference from examples. They have little or none of the compelling property which attaches to mathematical conclusions.

In other words, scientific (as opposed to mathematical) truth is not necessary truth. It is in the nature of things that the experimental scientist can not give us absolute truth. This is no criticism of his work; it is not his province to give us absolute truth —even if such a thing were supposed to exist.

What then is the purpose of the experimental scientist? His province is to enable us to get around among the phenomena of the external world, to predict what will happen under a given set of circumstances. He will accomplish this end by studying the relations among phenomena. He does not need to know their ultimate explanation; it is sufficient if he can find the essen-

tial threads of interconnection among them. Therefore he does not seek absolute certainty in his theories, at least when he realizes the fundamental limitations of his methods; but he understands his theories rather as the most convenient means by which he may summarize for himself and others the actually observed interrelations in nature.

Now, let us suppose that an experimental scientist attempts to attain absolute certainty in his conclusions, and enquire as to the kind of difficulty which he will encounter.

An analysis of the matter shows, first of all, that he must make one fundamental assumption—that involved in the hypothesis of the uniformity of nature. If phenomena have no laws it is futile to ascribe laws to them; and therefore a first requisite for the existence of experimental science is the supposition that laws exist. It must be assumed that the universe will not suddenly depart to-morrow from its previous way of behaving; it must not be a thing of caprice.

But what ground have I for believing that to-morrow will not put forth a set of phenomena totally different from those which I have observed before? None at all, except what comes through my belief in the uniformity of nature. It is clear that this is not the way by which the principle is to be established. In fact, we can go further and say with confidence that there is no absolute certainty, but only a high degree of probability, that nature is uniform.

There is also another fundamental assumption at the basis of experimental science—one that is curiously related to the mind that has made the assumption.

A fundamental property of mind is memory; without it mind can not exist in its usual state. What one does to-day is colored, modified, perhaps determined by one's memory of past acts. No experiment on a thinking subject can be performed for the second time; for the presence of memory in the second event is a factor of determining importance and can not be left out of account.

And yet mind, of which this is a characteristic and fundamental property, has chosen to assume that matter is without memory. If I desire to experiment with a falling stone, I need not enquire whether the stone has gone through the same experience before. In other words, I assume that the stone has no memory of its previous existence; and consequently its previous history will not affect my present experiment.

If it is true that experimental science is so shot through with basic assumptions, what is to be said of our confidence in its results? What measure of certainty attaches to them and how do we come to that certainty? Clearly, the evidence must be indirect; but it need not on that account be less trustworthy.

We may arrive at one phase of this evidence by noticing what change has taken place in man's relation to natural phenomena since the dawn of the modern era in scientific investigation. It is patent to every one that there has been an immense gain in control; man has harnessed the forces of the world and is using them for his purpose. A thousand and one new instruments of power and pleasure attest to his more profound understanding of the relations among phenomena. For hundreds of miles he can transfer the immense power of Niagara along a slender wire, and then use it to run his machinery and light his cities and warm his houses. In every conceivable direction he is making progress decade by decade; and the momentum of his progress increases as the years pass.

But even this is not the chief reason for believing that he is essentially right in his

interpretation of the relations of phenomena. His strongest ground of confidence is in the multiplicity and the accuracy of his predictions—predictions which he verifies by further tests in the laboratory.

Probably the severest test of a physical theory is the requirement that it predict accurately a phenomenon which has not yet been observed; and this is a test to which theory is constantly subjected—and it comes out successful. This is the ground of our confidence in physical theories. It is this which lends the strongest possible credence to such a general hypothesis, for instance, as that of the uniformity of nature.

This ultimate test of prediction finds its most extensive exemplification in the results obtained by the apparatus of abstract mathematical ideas. From a few fundamental laws, as for instance those of static electricity, an immense body of doctrine is built up by the processes of mathematical analysis. The results so obtained are exact and are stated with careful precision. Notwithstanding their great variety and the absolute precision with which they are stated, they are found to be always in accord with new experiment however the conditions may be varied. It is this which furnishes our strongest ground of confidence in physical theory; it is not the argumentation or inference by which the theory was first discovered or created.

The success of this prediction through mathematical or other argumentation is so great that we can not escape the conclusion that science is on the right track; improvements will come, to be sure, but we have certainly made some fundamental progress. In fact, the ground for this conclusion is so strong that the burden of proof must rest on whoever disputes its validity. If our theories are essentially erroneous, it requires careful explanation to understand why our attempt to put them in mathematical language has issued in such a remarkable success in the way of relating and predicting phenomena.

Even though we are still left face to face with the conclusion that there is no absolute certainty in our scientific theories, we see nevertheless that our ground of confidence in them is such as to justify our laying out our life and its activity as if they were so. We shall accept them as our guide in getting around among external phenomena. And we can do this even with more confidence than we can plan those things which depend on our own acts. Indeed there is much greater certainty attaching to the prediction of physical phenomena than to the prediction of our own acts; and what more could one reasonably demand of science?

Now of the two methods which we have considered, the mathematical and the experimental-scientific, which is the better? You will probably expect me to say that the mathematical method is the better; but I do not say it. Neither is the better; the question is meaningless. Each method is of profound importance and each is suited to its proper purposes; each will be improved as time passes and will be carried over more and more into all fields of thought and conduct; and each will continue to add new conquests to human achievement. But we shall not say that one is better than the other.

Most of you to whom I have spoken this evening are at the threshold of life. The future lies before you. You will doubtless choose some definite work to do in it. Would you like to have a part in promoting those fundamental ends of human development which may be secured through the use of one or the other of these great methods of advancement?

But what is it to have a part in using

and perfecting these tools, the two chief means by which mankind is making progress in our day? What sort of work is it? It is hard; it is no child's play; it is the work of maturity and strong purpose. The material rewards are few; probably not many of your generation will appreciate your labors, and most of you perhaps will not be heard of after your day. But you will leave mankind a heritage of profit forever, you will hasten the day when all men will know that their chief benefactors are those who delve into the secrets of nature and reveal them to their fellows. Does that work appeal to you?

R. D. CARMICHAEL

RECOLLECTIONS OF DR. ALFRED RUSSEL WALLACE

IT is impossible for any man to discuss adequately the life work of Alfred Russel Wallace. His activities covered such a long period, and were so varied, that no one living is in a position to critically appreciate more than a part of them. We are very much interested, of course, and have our opinions; but we need not pretend to any final or complete judgment. All must agree that a great and significant career has just been closed, but its full measure will probably never be known to any single man.

On the other hand, it may be possible to gain a clear idea of the character and aims of Dr. Wallace; and for our purposes this is perhaps the more important thing, since his guiding principles may also become ours, while the work he did is his alone. I once asked him about the origin of his interest in biology, and in the course of his reply[1] he said: "As to my interest in biology, . . . I doubt if I had or have any *special* aptitude for it, but I have a natural love for *classification* and an inherent desire to *explain things;* also a great love of beauty of form and color." Again, in writing to the biology students of the University of Colorado, he said:[2]

> The wonders of nature have been the delight and solace of my life. . . . From the day when I first saw a bee-orchis in ignorant astonishment . . . nature has afforded me an ever-increasing rapture, and the attempt to solve some of her myriad problems an ever-growing sense of mystery and awe.

This is the spirit of the amateur, using that word in its best and true sense. When Wallace had been long in the Malay Archipelago, a relative wrote urging him to return, and in his reply he gave the reasons why he could not do so, and said:

> So far from being angry at being called an enthusiast (as you seem to suppose), it is my pride and glory to be worthy to be so called. Who ever did anything good or great who was not an enthusiast?

This was his attitude to the end of his life, and only those who have some measure of the same feeling can understand it. The worldly wisdom of a professional threading his way through the maze of opportunity to one of the prizes of life was wholly foreign to his nature; he was, instead, the "irresponsible enthusiast," keenly anxious to see and know, *loving* nature and man, always wishing to communicate to others some of the pleasure and knowledge he had gained. To some his frequent advocacy of unpopular causes suggested perfect indifference to public opinion, and a total disregard of ordinary prudence. Whether, in this or that matter, we believe him to have been right or wrong, we must admire a man who always had the courage of his convictions; and so far from being indifferent to the feelings and opinions of others, his sympathetic nature and longing for fellowship *caused* him to so zealously expound what he believed would be helpful to other men.

I had of course revelled in "The Malay Archipelago" when a boy, but my first personal relations with Dr. Wallace arose from a letter I wrote him after reading his "Darwinism," then (early in 1890) recently published. The book delighted me, but I found a number of little matters to criticize and discuss, and with the impetuosity of youth, proceeded to write to the author, and also send a letter on some of the points to *Nature.* I have

[1] *Popular Science Monthly,* April, 1903, p. 517.

[2] SCIENCE, March 29, 1912, p. 487.

possibly not yet reached years of discretion, but in the perspective of time I can see with confusion that what I regarded as worthy zeal might well have been characterized by others as confounded impudence. In the face of this, the tolerance and kindness of Dr. Wallace's reply is wholly characteristic:

I am very much obliged to you for your letter containing so many valuable emendations and suggestions on my "Darwinism." They will be very useful to me in preparing another edition. Living in the country with but few books, I have often been unable to obtain the *latest* information, but for the purpose of the argument, the facts of a few years back are often as good as those of to-day—which in their turn will be modified a few years hence. You refer to there being five species of *Aquilegia* in Colorado. But have they not each their station, two seldom occurring together? During a week's botanizing in July in Colorado I only saw two species, *caerulea* and *brevistyla,*—each in their own area. Though the Andrenidae are not usually gaily colored, yet they are not *inconspicuous*. The Chrysididae are, I should think, colored so brilliantly, partly, perhaps, to simulate stinging species, and partly to prevent their being taken for fruits or seeds when rolled up. They are *very* hard, and like many hard beetles, are colored as a *warning of inedibility*. In the Rocky Mountains I think there is a *real scarcity* of Monocotyledons, especially bulbous Liliaceae and Amaryllids, and Orchises. This struck me as being the case. You appear to have so much knowledge of details in so many branches of natural history, and also to have thought so much on many of the more recondite problems, that I shall be much pleased to receive any further remarks or corrections on any other portions of my book.[3]

This letter, written to a very young and quite unknown man in the wilds of Colorado, who had merely communicated a list of more or less trifling criticisms, can only be explained as an instance of Dr. Wallace's eagerness to help and encourage beginners. It did not occur to him to question the propriety of the criticisms, he did not write as a superior to an inferior; he only saw what seemed to him a spark of biological enthusiasm, which should by all means be kindled into flame. Many years later, when I was at his house, he produced with the greatest delight some letters from a young man who had gone to South America and was getting his first glimpses of the tropical forest. What discoveries he might make! What joy he must have on seeing the things described in the letters, such things as Dr. Wallace himself had seen in Brazil so long ago!

It is comparatively easy for many of us to teach, as we do in schools. No doubt we communicate the "essentials" of our subjects in a fairly competent manner; but would that we had in this country more grand old men with the will and right to bless the succeeding generations as they come.

Some letters of August and September, 1890, refer to a suggestion of mine that a collection of all the recorded facts bearing on evolution should be made.

The proposal you make of a collection of all the recorded facts bearing upon the various problems of *Darwinism* is a very good one. Such a body of facts would be most valuable to naturalists, but I question whether it would pay for its publication. I feel sure my publishers would not agree to "weight" my book with such a mass of additional matter. The only thing, therefore, would be to publish the materials separately, as Darwin did in his "Animals and Plants under Domestication." I hope *you* will do this yourself, as you have evidently a taste for this kind of work. . . . It would, however, be a tremendous task, as it would involve wading through the *whole* literature of natural history for the last twenty years.

In a second letter:

If half a dozen workers could be found to undertake the work of collection I should think the Royal Society would give funds for the publication, as the work would be really a *supplement* to *Darwin's works,* and might be suggested as a *Literary Memorial* to him.

The project was never even on the way to be carried out, owing to various circumstances. I believe it might even now be begun, and that it would be well worth while. For example, we have no good collection of data concerning the relations between specific characters and locality, or on the relative frequency of variation in different species, and a number of other

[3] Letter, February 10, 1890.

equally interesting topics. One constantly reads good papers on experimental work, which suffer from the almost total ignorance of the authors concerning the variability and different specific characters of the genera they are dealing with. Not only could much that is valuable be obtained from the literature, but the museums are full of materials which on examination would yield a rich harvest.

Dr. Wallace was greatly impressed with the waste of opportunity in our museums, and not very long ago (Sept. 30, 1909) wrote urging that something should be done.

If you can find time I wish you would write to "Nature"—or if at more length to the "Fortnightly Review"—on a matter of great importance to the philosophical study of biology. Our vast accumulations of plants at Kew, and of insects at the Natural History Museum contain a mass of most valuable geographical and statistical information, quite *lost, useless* and *unknown*, owing to the absurd system of devoting all the time and energies of the staff of curators, etc., to describing new species or small groups here and there, or publishing a few enormous and very costly works like Sharpe's Catalogue of Birds, —which, though intrinsically of great value, are lost to the *mass of workers* owing to *cost* and bulk. Thiselton-Dyar wrote me lately that he "*groans* over the masses of material which lie useless and unknown at Kew." I have urged the last and present Directors of the Natural History Museum to devote their influence to making a simple *Catalogue* of the Museum contents, beginning with the richest and most popular families or sub-orders of insects—Longicorns, Carabidae, Cicindelidae, Lamellicornes, etc., also *Diurnal Lepidoptera.* This catalogue or list, could be made by intelligent *clerks* only, by going over the cabinets or cases, in systematic order, and entering every *specific name* (or sp. nov.) and the *numbers* of the *specimens* in the Museum from *each separate locality.* The clerk or clerks would be under the general supervision of the Curator of the special department. From this manuscript list, a card-catalogue should be set up and stereotyped; there being a card for each species and named variety, and in the case of all wide-spread species, separate cards for each *Continent* or each considerable *Country.* By printing several *sets* of these cards, a card-catalogue for any sub-family or genus, or for any *geographical region* or *country,* could be made up at a *low price,* and would be invaluable to all private collectors, as telling them at once what *is* in the B. M., and *where from,* while the number of *specimens* would be some guide to the abundance or rarity of the species. I am immensely impressed with the value of the plan of *Card Catalogues,* so much used in America, but I suppose almost unknown here except for Libraries. I have no time or strength to go into this subject properly. . . .

Dr. Wallace had not seen some of the more recently published works, in which such information as he desired had actually been given; but it was and is true that all large museums might do much more for the advancement of biological science, were they to fully utilize the materials at their command. The greatest objection to catalogues compiled in the manner suggested is that the determinations of specimens are frequently unreliable, so that expert revision of the several groups would be necessary in the first place. This means more curators, and therefore more expense. It is however a very wasteful policy, which would wreck any private business, to keep up a large museum at enormous cost, and then cut off the funds at the point of providing an adequate staff to take care of the contents. It is as though a large department store were furnished with everything except enough clerks and salesmen to attend to the customers. Several curators of the U. S. National Museum, to whom I put the question, concurred in the opinion that 5 per cent. added to the total cost of running the museum, put into expert curators, would double the scientific output. In addition to taxonomic workers, museums ought also to have men with broad interests like those of Dr. Wallace, whose business it would be to survey and expound the facts relating to geographical distribution, variation, etc., obtainable from the collections. Thus at the British Museum, Hampson's great work on the moths of the world might be made the basis for many interesting generalizations, which would interest and instruct many who could not obtain or read the original severely taxonomic volumes.

In October, 1890, after I had returned to England, Dr. Wallace wrote that he was about

to prepare a new edition of his "Island Life," and asked me to help secure the information necessary to bring it up to date. I of course gladly agreed to do this, and was supplied with the loose sheets of the first edition, which I carried to the British Museum (Natural History) and the library of the Zoological Society, comparing the chapters with recent literature, and especially consulting different naturalists on their specialties. This not only proved extremely interesting work, but it gave me an introduction to many men I had wished to meet, and especially brought me into constant communication with Dr. Wallace himself. All who were approached courteously gave the best aid in their power; but one chapter, that on the British Islands, proved quite a bone of contention. Dr. Wallace had given lists of animals and plants peculiar to those islands, enumerating all the species and varieties which appeared not to have been recorded from elsewhere. He argued that while no doubt these lists required amendment, yet it was probably true that we possessed a considerable series of endemic forms. Almost without exception, the naturalists of that time expressed great scepticism on this point, while some freely ridiculed the whole idea. Even when furnishing data, they hastened to say that they were probably of no value. Since that time, careful collections have been made by British naturalists on the continent, and much work of various kinds has been undertaken which bears directly upon the question of an endemic element in the British fauna. The result has been to reveal an amount of divergence far in excess of Dr. Wallace's expectations; so much so, that when a few years ago I mentioned to him the recent results of mammalogists, he was not himself prepared to go so far, but said they surely must be splitting hairs.

Early in 1891 I went down to Parkstone and had the great pleasure of meeting Dr. and Mrs. Wallace. For about a week I spent a large part of each day at Dr. Wallace's house and sometimes went for walks with him. I now regret that I kept no notes of the conversations, but I recall that we discussed all the debatable biological and sociological questions of the day. More especially, we talked about the inheritance of acquired characters, and tried to postulate crucial experiments to prove the matter one way or the other. We found it extremely difficult to even imagine an experiment which should be above all possible criticism. There was also much to be said about geographical distribution; and just at that time I had published some remarks on alpine plants in *Nature*, which had called forth adverse criticism, to which I replied while at Dr. Wallace's house. I remember that he encouraged me to go forward in this matter, and not mind if people said I was out of my proper department. He believed in, and of course illustrated by his own conduct, the right of any man to study what he chose, and not be limited in his intellectual activities because his colleagues had labelled him this or that.

After my return home we continued to discuss the inheritance of acquired characters through the mails, especially since at that time Dr. Romanes and others had on foot a project for an experimental station. The following is from a letter of February 7, 1891:

Your former letter (of Feb. 2) giving Romanes' reply to you, set me going and I immediately wrote to Galton. I enclose his reply, which please return when you are writing next. I then sat down and sketched a series of a dozen sets of experiments to test the two questions of "*heredity of acquired characters*" and the "amount of sterility in the hybrids between closely allied species," —and also a few to test the questions of instinct in nest building, and the "homing" power of dogs, cats, etc. These I am now sending to him and shall then receive his objections to them as affording tests. In the mean time will *you* try and formulate a few experiments which would serve as *crucial tests* of the question of the "heredity of individually acquired characters?" You may hit on some that will meet the objections he will probably make to mine. I do not think there will be any difficulty in getting good observers in paid servants under the supervision of a committee.

On February 13 Dr. Wallace reported the receipt of a long letter from Galton, criticizing some of the suggested experiments. The letter continues:

I suggested some experiments something like yours, and many others. I do not quite agree with you that if acquired characters *are* inherited, they might only be so very rarely. If *inherited* (to be of any use in the theory of evolution, and *that* is the whole question) they ought to be inherited as frequently as *other* characters are inherited, that is, I presume, in about *half* the offspring. If only one in 100 exhibited the character how could you possibly say it was not a normal variation in that individual? *Only* by the very frequent inheritance could you *prove* that there was any inheritance at all! I think you will see this. But it is too elaborate a question to discuss in letters.

On February 18, however, he discussed the matter at greater length:

As you are a student of *variation* I thought you would see my point without explanation. Now I will explain. The following three points I consider to be proved by overwhelming evidence, a summary of which is given in "Darwinism," Chap. III.

1. All increasing or dominant species (and it is from these that new species arise) vary considerably, in all their *parts, organs* and *faculties*, in *every generation.*

2. The amount of this variation is so large that when only 20 to 50 adults are compared it reaches from 10 to 20 per cent. of the mean value of such characters as can be accurately measured.

3. The proportion of individuals which vary considerably is large, reaching to one fourth, or one third of the whole number compared. In other words, the curve of variation is low. . . .

Hence it follows that *whatever character* is increased or diminished in individuals by the effect of the environment, a similar increase or diminution will occur by genetic variation, in each generation, and in certainly 5 or 10 per cent. of the individuals dealt with. Hence your supposition that in the check lots *no* such modification would occur as in those exposed to special conditions is almost an impossible one; and an effect produced on *one* or even on *five* or 10 per cent. by special conditions would be imperceptible, because similar effects would occur through normal variation and often to a much greater amount. Hence I said, that to be *clear* and *decisive* the effect produced by the conditions should be inherited by a *large proportion* of the offspring. You may say that the effects of conditions would be *additional* to the normal effects of variation. True. And if largely inherited they would soon show it, but if as you first supposed only one per cent., that would be entirely swamped by the irregularities of normal variation and inheritance. You must remember too that experiments on a very large scale, and with check experiments on an equally large scale, and all carried on for many years, would require a very large establishment and ample funds not likely to be obtained. Again, the whole *raison d'être* of this enquiry is to decide whether inheritance of acquired characters is of any importance in the origin of species. To be of importance it must rank in generality with variation, otherwise it is entirely superfluous, even if it exists, and variation could do perfectly well without it. Yet again, either there is a fundamental cause of such inheritance or there is not. If there is,—if such inheritance is a *law of nature*, why should it not rank with the inheritance of genetic variations?—which are, I presume, to the extent of about *one half?* If it was only one per cent., it might be a fluke! It would require innumerable experiments to prove it was anything else.

I have given this discussion partly to show that even in those days there was much talk of experimental work, and that the necessity for such work was fully appreciated. Dr. Romanes prepared a statement, which was widely circulated, urging that an experimental station should be established at Oxford or Cambridge,[4] but the funds were not forthcoming. We thought at one time that Oxford would rise to the occasion, but she failed to do so, and it was long after that Cambridge established a chair of genetics.

During the winter I unsuccessfully competed for a position in the Marine Biological Station at Plymouth, and Dr. Wallace kindly interested himself on my behalf. When, in April, I was appointed curator of the museum of the Institute of Jamaica, I had reason to believe that Dr. Wallace had a good deal to do with the matter, since he evidently knew all about it before I told him. He wrote me a charming letter of congratulation:

How you will revel in the land Molluscs, and how you will punish the poor slugs who have hitherto been unregarded by collectors! . . .

[4] "Life and Letters of George John Romanes," second ed. (1896), p. 269.

You will also be able to have a garden, and to be within easy reach of the higher ranges of mountains where hosts of new insects and molluscs remain for *you* to discover! As *you* will treat the poor niggers as "men and brothers," you will have no difficulty in getting any servants you require. . . .

In the following year Dr. Wallace himself thought of visiting Jamaica, and wrote:

Should you see any nice little cot to let in some nice place in the mountains, with plenty of rock and forest near by, let us know, and if we can let our house here for 6 months we *may* possibly come and be renovated by the glorious sun of Jamaica.

In 1893, after I had gone to New Mexico, Dr. Wallace wrote (Sept. 10):

I and wife went to the *Lakes* for a month in July and August,—our first visit there. I was delighted both with the scenery and the glacial phenomena. The mountains are very precipitous, with fine bold outlines and grand precipices, and their summits, at 3,000 feet, quite as grand examples of mountain structure and of denudation as 12,000 or 14,000 feet peaks in the Rockies!

The years passed by, bringing good and ill fortune, and it was not until June, 1904, that I again saw Dr. Wallace. He had moved from Parkstone to Broadstone, where he had built a house in an ideal spot, surrounded by a beautiful garden, and with a small greenhouse annexed. Adjacent to the garden is a sort of miniature forest; "this," he said, "we call the tulgey wood." Every morning he went out early, to see what flowers had opened, and to pick the strawberries. His enthusiasm over the flowers was unbounded; as he himself said, the passage of years had increased instead of dulling his love of natural beauty. We were shown the new hybrid roses, and especially the rockeries, where many beautiful alpines were growing to perfection. One day we all went to Corfe Castle, and Dr. Wallace, in spite of his age, was able to climb the hill on which that ruin stands, and examine every part of it.

In subsequent years my wife and I frequently heard about the garden, sometimes from Dr., sometimes from Mrs. Wallace. They sent us seeds of *Anchusa* and old-fashioned English pinks, which have done very well in our garden at Boulder; we sent *Rosa stellata* and the new red sunflower, both of which were first grown in England by the Wallaces. On June 26, 1911, soon after the publication of "The World of Life," Dr. Wallace wrote:

After the hard labor of my book, and the flood of correspondence about it, chiefly from admirers, —I am taking relaxation in a new *rock and bog garden*, which I have been making, and especially in growing as many as I can of the lovely genus *Primula*, especially the fine new species recently discovered in the mountains of *China* and the *Himalayas*. These I am growing as much as possible from seed, as their beauty is only shown in groups or masses; and I have already got altogether about 40 species (chiefly presents from Kew, Edinburgh, Dublin, etc.). I am very anxious to get *your* very remarkable and fine *Primula Rusbyi* from New Mexico, and in the hope that your university may have a botanical garden, or that some of your botanists may grow it; I shall greatly prize some *seed* gathered and posted in a letter as soon as the capsules are mature. Seed of the Californian *P. suffruticosa* and the Coloradan *P. Parryi* will also be very welcome, as well as of any other American species, if such there are.

P. rusbyi I had never obtained at any time; the allusion to my species was probably due to some recollection of the equally fine *R. ellisiae*, which it was impossible to procure. We did, however, obtain some roots of *P. parryi*, and Dr. Wallace wrote:

I have received a very nice little parcel of *fine roots* of the handsome *Primula Parryi*, which I saw growing luxuriantly near Kelso's cabin, below Gray's Peak, at 11,500 feet, and which I hope to see in flower again next spring, as I have given it a place where it can get its *roots in water*, as it did there, on the margin of the stream.[5]

In the same letter he says:

About two months back was much surprised and pleased to have a visit from Miss Eastwood, my companion in our trip to *Gray's Peak* and *Grizzly Gulch*, in July, 1887, where we saw the American Alpine flora at the snow-line in perfection.

Then again:

Answering letters, reading the papers, mags. and books, with a lot of *novels* fills up my time,

[5] Litt., December 17, 1911.

with attention to my Alpines and seedling Primulas, though I have promised to write an important article, when I feel up to it, *"On the Influence of the Environment on Morals."* We are having the dullest, dampest and dreariest winter I remember, after the hottest summer! . . . The political and foreign situation is now most interesting with us, and I am glad to have lived to see such a hopeful dawn.

The last time I saw Dr. Wallace was immediately after the Darwin Celebration at Cambridge in 1909. I was the first to give him the details concerning it, and vividly remember how interested he was, and how heartily he laughed over some of the funny incidents, which may not as yet be told in print. One of Dr. Wallace's most prominent characteristics was his keen sense of humor, and his enjoyment of a good story. At the banquet at Cambridge those present united in sending him a telegram expressing their sense of his great part in the event they were celebrating, and their regret that he could not be present. This was not delivered until the next morning, and Dr. Wallace was concerned lest it should have been thought that he delayed in sending a reply. I was able to assure him that we knew at the time that it was too late for delivery that day.

As recently as February 3, 1913, Mrs. Wallace wrote:

Dr. Wallace is very well and busy, writing as hard as ever; he has just passed 90, and feels like 50.

Much later in the year (July 1) we heard from my brother that he was "splendidly well," and not many months after, the sad news appeared in the daily papers. In one of his letters he said that except for the infirmities natural to old age he felt quite as keen as he had ever done in his youth, and thought this a good sign for the persistence of personality after death. This keenness never waned to the end, and who shall say that this eager spirit has not still some place in the realm of being?

T. D. A. COCKERELL

SCIENTIFIC NOTES AND NEWS

THE Nobel prizes in the sciences have been awarded to Professor H. K. Onnes, of the University of Leiden, in physics; to Professor Alfred Werner, of the University of Zurich, in chemistry, and to Professor Charles Richet, of the University of Paris, in medicine.

AT the anniversary meeting of the Royal Society Sir William Crookes was elected president to succeed Sir Archibald Geikie. Other officers were elected and prizes were conferred as already announced in SCIENCE. At the annual dinner the principal toast, "The Royal Society," was proposed by Mr. Page, the American ambassador. The retiring president announced a gift of £5,000 for physical research from Sir James Caird.

DR. J. H. COMSTOCK, for thirty-nine years instructor and professor of entomology at Cornell University, will retire from the active duties of his chair at the close of the present academic year.

DR. HERMAN M. BIGGS has retired as chief medical officer of the Department of Health of the City of New York, having rendered distinguished service to the city in that office.

PROFESSOR CLEVELAND ABBE, the distinguished meteorologist of the U. S. Weather Bureau, celebrated his seventy-fifth birthday on December 4.

THE gold medal of the Apothecaries Society, London, has been awarded to Mr. J. E. Harting, in recognition of his services in preparing and editing the catalogue of the library in Apothecaries' Hall.

THE portrait of Professor Horace Lamb, F.R.S., was presented on November 27 by subscribers to the University of Manchester, where he has filled the chair of mathematics since 1885, and is now senior professor. The portrait of Professor Lamb was painted by his son, Mr. Henry Lamb. The presentation was made by Professor Tout and Professor Rutherford.

DR. CHARLES S. MINOT has been elected an honorary member of the Anatomical Society of Great Britain and Ireland.

At a special meeting of the Royal Spanish Society of Natural Science held in Madrid on November 28, Dr. W. J. Holland, the director of the Carnegie Museum in Pittsburgh, was elected an honorary member to fill the vacancy in the list of honorary members created by the death of Lord Avebury. At the same meeting Mr. Arthur S. Coggeshall, of Pittsburgh, was elected a corresponding member of the society.

Professor R. W. Wood, of the Johns Hopkins University, who is spending the year abroad, is engaged in research work in the laboratories of the Sorbonne and the Ecole Normal Superieur (Paris) in collaboration with Hemsalech, Dunoyer and Ribaud. His address is 14 Ave. Charles Floquet, Paris.

At the annual meeting of the Entomological Society of Washington, held on December 4, 1913, the following officers were elected: *President,* W. D. Hunter; *First Vice-president,* A. N. Caudell; *Second Vice-president,* E. R. Sasscer; *Editor,* W. D. Hunter; *Corresponding Secretary-Treasurer,* S. A. Rohwer (U. S. National Museum, Washington, D. C.); *Additional Members of the Executive Commitee,* Dr. L. O. Howard and Messrs. E. A. Schwarz and August Busck. These officers will be installed at the first meeting in January.

Mr. N. Cunliffe, B.A., Trinity College, has been appointed assistant to the superintendent of the Museum of Zoology of Cambridge University.

Dr. Sebastian Albrecht has been appointed astronomer at the Dudley Observatory, Albany.

E. J. McCaustland, professor of municipal and highway engineering at the University of Washington, Seattle, has been appointed by the county commissioners as consulting engineer for King County. In conjunction with the state highway commissioner Mr. McCaustland will act as adviser to the county engineer in the expenditure of three million dollars for permanent highways.

At the regular fall meeting of the Chicago chapter of the Sigma Xi held on December first, the society was addressed by Professor Jacques Loeb, of the Rockefeller Institute for Medical Research, who spoke on "Recent Experiments in Artificial Parthenogenesis."

During the week of December 1–6, Professor Lafayette B. Mendel, of the Sheffield Scientific School of Yale University, gave addresses on "Viewpoints in the Study of Growth" and "Food Fads" before chapters of the Sigma Xi society at the University of Kansas, University of Missouri and Washington University in St. Louis.

Professor Douglas W. Johnson delivered the following series of illustrated lectures on "The Interpretation of American Scenery" before the Institute of Arts and Sciences of Columbia University on Saturday evenings during the month of November: The Scenery of American Rivers; Shoreline Scenery of the Atlantic Coast; The Sculpture of Mountains by Glaciers, and the Scenery of the Grand Cañon District.

On the evening of November 14, Professor W. W. Atwood, of Harvard University, presented an illustrated lecture to the Geographic Society of Chicago on "The Ascent of Uncompagre and a Trip through the San Juan Mountains of Colorado."

Professor Arthur H. Blanchard, of Columbia University, on December 6, delivered an illustrated lecture on "Modern Developments in Highway Engineering," before the Drexel Institute of Philadelphia.

Dr. Wolfgang Ostwald, Privatdozent at the University of Leipzig, editor of the *Kolloid-Zeitschrift* and the *Kolloidchemische Beihefte,* and known for his many scientific contributions to biology and chemistry, has been invited by the Cincinnati branch of the American Chemical Society and the Cincinnati Research Society to give a series of five lectures on colloid-chemistry in the University of Cincinnati during the week of January 5 to 10. The lectures embrace a discussion of the general properties of colloids with scientific and technical applications. In the week of January 12 to 17 these lectures will be repeated at the University of Illinois; January 19 to 24

at Columbia, January 26 to 31 at Johns Hopkins; February 2 to 7 at the University of Chicago.

THE Huxley lecture at Birmingham University for this year is to be delivered by Sir Arthur Evans, F.R.S., who has chosen as his subject "The Ages of Minos."

THE Swiney lectures on geology in connection with the British Museum (Natural History) are being given this year by Dr. T. J. Jehu, his subject being "The Natural History of Minerals and Ores."

PROFESSOR ALFRED G. COMPTON, former head of the physics department at the College of the City of New York, who retired in December, 1911, after serving on the faculty of the college for fifty-eight years, died on December 12, aged seventy-eight years.

DR. JAMES MACALISTER, for twenty-two years president of the Drexel Institute at Philadelphia, and previously superintendent of public schools, died on December 11, at the age of seventy-three years.

PROFESSOR DR. ANTON FRIČ, one of the most distinguished of the paleontologists of Europe, died in Prague on the fifteenth of November, in the eighty-first year of his age. Professor Frič's greatest contributions were to the Permian fauna of Bohemia, especially the Amphibia and fishes, and also the insects. He has also left a permanent record in his direction of the beautiful natural history museum at Prague which is in many respects the most perfect of its kind in Europe. He was a man of very great energy and a voluminous writer. His published works include many large volumes which will become classics in paleontological literature.

PROFESSOR IGINO COCCHI, of Florence, known for his work in stratigraphical geology, the first president of the committee directing the Geological Survey of Italy, has died at the age of seventy-five years.

THE foundation-stone was laid on November 23 at Frankfurt-on-Maine of the new zoological institute of the Senckenberg Natural History Museum which the Senckenberg Society will ultimately place at the disposal of the future University of Frankfurt.

THE thirty-first German Congress of Internal Medicine will be held at Wiesbaden, April 20–23, under the presidency of Professor von Romberg, of Munich. The chief subject proposed for discussion is the nature and treatment of insomnia. The reporters are Drs. Gaupp, of Tübingen; Goldscheider, of Berlin, and Faust, of Würzburg.

THE committee charged with the local arrangements for the recent visit to Birmingham of the British Association has held its final meeting. It was reported that the number of persons taking tickets for the meeting was 2,635, compared with 2,504 at the Dundee meeting last year and 2,453 at the Birmingham meeting in 1886. The extent to which the artisan classes availed themselves of the popular science lectures made them a notable feature of the meeting. The Finance Committee recommended that an unexpended balance of £2,313 be returned to the contributors proportionately.

A PRIZE of one hundred dollars is offered for the best paper on "The Availability of Pearson's Formulæ for Psychophysics." The rules for the solution of this problem have been formulated in general terms by William Brown. It is now required (1) to make their formulation specific, and (2) to show how they work out in actual practise. Papers in competition for this prize will be received not later than December 31, 1914, by Professor E. B. Titchener, Cornell Heights, Ithaca, N. Y. Such papers are to be marked only with a motto, and are to be accompanied by a sealed envelope, marked with the same motto, and containing the name and address of the writer. The prize will be awarded by a committee consisting of Professors William Brown, E. B. Titchener and F. M. Urban. The committee will make known the name of the successful competitor on July 1, 1915.

PARTICULARS of the Pierre J. and Edouard Van Beneden prize of 2,800 francs are quoted in *Nature*. The prize is to be awarded every three years to the Belgian or foreign author

or authors of the best original work of embryology or cytology written or published during the three years preceding the date on which competing theses must be received. For the first competition this date is December 31, 1915. The manuscript works may be signed or anonymous, and the French, German, or English language may be employed. Authors should send their contributions to the permanent secretary of the academy, Palais des Académies, Brussels, inscribed "Concours pour le Prix Pierre-J. et Edouard Van Beneden."

President Wilson, in his annual address to members of Congress, referred to the United States Bureau of Mines in the following manner: "Our Bureau of Mines ought to be equipped and empowered to render even more effectual service than it renders now in improving the conditions of mine labor and making the mines more economically productive as well as more safe. This is an all-important part of the work of conservation; and the conservation of human life and energy lies even nearer to our interest than the preservation from waste of our material resources."

The British home secretary has appointed a committee to inquire what action has been taken under the Wild Birds Protection Acts for the protection of wild birds and to consider whether any amendments of the law or improvements in its administration are required. The members of the committee are: The Hon. E. S. Montagu, M.P., under-secretary of state for India (chairman); Lord Lucas, parliamentary secretary to the board of agriculture; Mr. Frank Elliott, of the home office; Mr. E. G. B. Meade-Waldo, Mr. W. R. Ogilive Grant and Mr. Hugh S. Gladstone. The secretary to the committee is Mr. H. R. Scott, of the home office.

The annual inspection trip of the department of electrical engineering of the University of Illinois took place November 23–26. The trip was under the charge of Professors E. B. Paine, Morgan Brooks, E. H. Waldo and J. M. Bryant. The party was divided into two sections. One section visited the Keokuk water power plant, while the other visited the industries around Joliet, Illinois. The sections met in Chicago, where the trip was concluded. Features of the trip were the inspection of the parts of the Commonwealth Edison system in Chicago, the Hawthorne works of the Western Electric Company and the Illinois Steel Works.

UNIVERSITY AND EDUCATIONAL NEWS

Announcement is made at Yale University that the new biological laboratories are to be called the "Osborn Memorial Laboratories." The funds, amounting to half a million dollars, were provided for in the will of the late Mrs. Miriam A. Osborn. The laboratories accommodate the departments of zoology, comparative anatomy and botany.

Regulations for admission to the military academy at West Point have been modified so that without lowering the entrance requirements prospective cadets may be matriculated by substituting equivalents for some of the units of study hitherto insisted upon. Hereafter a candidate for admission may be excused from mental examination upon presentation of certificate that he is a regularly enrolled student in good standing in a university, college or technological school, the entrance requirements of which include proficiency in mathematics and English as outlined by the college entrance examination board, or a certificate that he has graduated from a preparatory school meeting the requirements of that board, or a certificate that he has passed fourteen units of the entrance examinations required by the board requiring mathematics, English and history.

Recommendation has been made to the Argentine Congress to send to America for two years' study at government expense two professors from each faculty of each national university.

Mrs. Ella Flagg Young has resigned as superintendent of Schools of the City of Chicago because certain members of the board voted against her re-election. It is now said

that these members of the board have resigned and that Mrs. Young may accept the election.

DR. LIVINGSTON FARRAND, professor of anthropology in Columbia University, has been elected president of the University of Colorado.

PRESIDENT THOMAS F. KANE, of the University of Washington, was removed from office on December 12 by the board of regents, who unanimously adopted a resolution declaring the office vacant. The action was the climax of an agitation that has lasted three years, in which a majority of the faculty and students are said to have aligned themselves against President Kane.

AMONG new appointments at the University of Montana are: N. J. Lennes, Ph.D. (Chicago), instructor in Columbia University for the past three years, to be head of the department of mathematics, and A. George Heilman, M.D. (Pennsylvania), to be instructor in biology and physiology.

DR. W. T. GORDON has been appointed lecturer and head of the geological department at King's College, London, in succession to Dr. T. F. Sibly, appointed professor of geology at the University of South Wales, Cardiff.

DR. G. OWEN, lecturer in physics at Liverpool University, has been appointed professor of physics at Auckland University College, New Zealand.

DISCUSSION AND CORRESPONDENCE

MORE PALEOLITHIC ART

BY degrees paleolithic stations are being rediscovered. The large rock shelter of La Colombière, valley of the Ain, some thirty miles southwest of Geneva, is an example. Known since 1875 it had been only superficially explored. The important discoveries of Dr. Lucien Mayet, of the University of Lyons, and M. Jean Pissot, of Poncin, date from October, 1913; and were first announced through the Paris Academy of Sciences on October 20. The trench they dug revealed in section: (1) neolithic at the top; (2) a Magdalenian horizon, the upper section of which with the neolithic had been disturbed by earlier investigators; (3) a layer of fine sand with débris from the overhanging rock, one meter thick, in which no relics were found, representing a long period of non-habitation by man; (4) Aurignacian layer with fossil remains of the mammoth, woolly rhinoceros, reindeer and horse. Here also was a workshop left by Aurignacian man, flint tools and rare engravings characteristic of the epoch.

The principal find is a large fragment of mammoth bone on which are engraved human figures; a head and upper part of the body including an out-stretched arm and hand; likewise a figure with head and feet missing, probably a female. Both these engravings are in profile, the view easiest to master by a primitive artist working in outline. Fairly good examples of the human form in the round and in relief dating back to the Aurignacian epoch are already known. Engraved figures are rare and so far as the head is concerned are little more than caricatures. The example from La Colombière is no exception in this respect and curiously enough resembles certain engraved human heads previously reported, one from the cavern of Font-de-Gaume (Dordogne), one from the Grotte des Fées (Gironde), and others from Les Combarelles (Dordogne) and Marsoulas (Haute-Garonne). In the Aurignacian layer were also found pebbles with engraved figures of the bison, *Felis,* horse, and wild sheep. When it is recalled that four fifths of all Quaternary engravings are animal figures, the bison and horse predominating, the importance of these two human figures from La Colombière at once becomes evident.

GEORGE GRANT MACCURDY

YALE UNIVERSITY

ON INTERFERENCE COLORS IN CLOUDS

THE writer has, for some time, noticed certain colors in clouds as they pass near the sun, and more careful observation indicates that an interesting effect is present which may not hitherto have been described. If the clouds within an angle of 15°, or so, from the sun are examined carefully, the sun, itself, being hidden by the corner of a

building or the roof of a piazza, certain parts of thin clouds, or edges of thick clouds, will usually be seen tinged with red or green, the colors often appearing together with red predominating. Occasionally the tint will be straw-color or purple. The effect may be seen at any time during the day, preferably when the sun is at a considerable elevation above the horizon. The colors are seldom intense, but are, nevertheless, very beautiful. They may be distinguished, when faint, by comparing them with any white cloud at an angle of 30° or 40° from the sun.

As the clouds in question are very brilliant, one's eyes have to become accustomed to the glare before the colors can be seen. Hence it is better to use smoked glass or dark glasses.[1] A smoked glass plate, on which the density of the smoke deposit varies from one edge to the other, is very convenient, as the best density for any particular cloud may quickly be found.

The following facts indicate that the mechanism of the effect is totally different from that by which the rainbow is produced. The colors appear in irregular patches of various sizes, and not in arcs of circles concentric with the sun. In fact, two small clouds may be close together, one being colored while the other is pure white. The red and green do not always appear together, the red occurring alone more frequently than the green. The same portion of cloud will frequently change from one color to the other.

It seems most reasonable to attribute these colors to interference. To make this clear, consider what must happen when white light passes through a water drop or ice crystal. At the surface where the light emerges, the ray will be divided, part passing through, and part being reflected back, to be reflected from the upper, or incident, surface of the drop, thence passing out through the lower surface. This second part will afford interference with the part of the ray that passed through unreflected, for a certain wave-length, provided a sufficient difference of phase, between the two parts of the ray, has been introduced. Owing to the shape of the drop, or particle, only one particular ray will, after undergoing this division, have *both* these parts sent in the direction of an observer on the ground (just as in the rainbow, each drop behaves like a prism, to an observer, but only for light that passes through one particular plane). If, further, we suppose that there are many drops of very closely the *same diameter*, then an observer should see light of the same color as that transmitted through a thin film, *e. g.*, a soap film or thin mica, of a thickness equal to this diameter.

Certain evidence supports the above explanation. The phenomenon is especially prominent in clouds that are increasing or decreasing in density. For example, in one particular cloud that was observed, which was increasing in size, the edge was first red, then green, then gray. Further, a cloud was occasionally seen with the red and green arranged in three or four alternate bands, strikingly suggestive of Newton's rings, or the fringes produced by an interferometer.

If the explanation here given is correct, these colors, besides of interest as being possibly the only sky colors produced by interference, may also be of some meteorological importance, namely; in giving an idea of the degree of homogeneity of size of drops in portions of thin clouds, by the intensity of the color; of the extent of these portions, by the area occupied by the color, and of the size of the drops, by the particular color present. Perhaps more information could be obtained by a spectroscopic method, whereby the spectrum of a small portion of cloud would show dark bands, corresponding to the wave-lengths removed from the light by interference.

Robert H. Goddard

Worcester, Mass.,
November 2, 1913

[1] A solution of a substance, having transmission bands in the red and green only, would be best for observing the colors most frequently seen, namely, red and green.

ORIGIN OF MUTATIONS

Gates, in a personal letter, has kindly called my attention to a misstatement contained in

my note[1] regarding the possible origin of mutations in somatic cells, in which I erroneously credited to Davis[2] the suggestion that triploid (semi-*gigas*) mutants of *Œnothera* are to be accounted for through the production of occasional diploid gametes by an extra fission of chromosomes. Obviously, as Gates points out, Davis's suggestion of diploid gametes could not have been offered as an explanation of triploid mutants, for the reason that the triploid condition in *Œnothera* was not known in 1911. Davis's suggestion was offered to account for the tetraploid condition of *gigas* mutants. The suggestion that tetraploid mutants may arise through a double fission of chromosomes in some mitosis soon after fertilization should have been credited to Gates.[3] I am grateful to Gates for setting me right in these matters.

R. A. Emerson

University of Nebraska

1 *Amer. Nat.*, 47: 375, 1913.

2 *Annals of Botany*, 25: 959, 1911.

3 *Archiv f. Zellforsch.*, 3: 525, 1909.

HOW ORYCTES RHINOCEROS, A DYNASTID BEETLE, USES ITS HORN

Many beetles, particularly in the family Dynastidæ, have more or less conspicuous horns or processes on their head or prothorax. These often assume fantastic shapes and enormous proportions. Sometimes they occur on both sexes, but more often they are found only on the male or at least reach their greatest development there. In the latter case they have been looked upon by some as characters that may have been developed through sexual selection, the assumption being that males so ornamented were more attractive to the females or in some other way were more likely to be able to mate and thus perpetuate their kind. While such a theory may not be very satisfactory without more detailed observations or experiments to prove its soundness, we know of no other that is any more acceptable.

Many of the horns and projections are of such a size and character that it is hard to conceive of their being of any possible use to the insect in its struggle for food, or with its enemies. Possibly some of them are of no use in this way, but while studying the rhinoceros beetles, *Oryctes rhinoceros*, in Samoa last summer, I had an opportunity to watch these insects making a very evident and profitable use of the horn on their heads. The horn is present on both sexes and is usually longer on the male than on the female, but many males may be found with very short horns and many females with long horns, so that the sexes can not be separated by this character. The horns vary in length from 1.5 mm. to 10 mm., 6 or 7 mm. being about the average length. The beetles feed on the growing heart in the crown of the coconut trees. They usually enter the trees close to the base of a leaf, crawling down as far as they can between the tree and leaf-stem before beginning to bore. The spiny legs enable the beetle to brace itself firmly before it begins literally to root its way into the web-like sheath through which it usually has to pass before it reaches the hard wood. In doing this the head is lowered and the horn thus thrust forward. The horn becomes imbedded in the tissue of the plant and when it is raised serves as an anchor to hold the insect while it pulls or pushes its body forward with its legs, or while it tears the tissue of the plant with its heavy mandibles. The insect will always root and push its way as deep as it can before it begins to bore. The amount of power it can develop while trying to force its way between the bases of two leaves or in other tight places is truly remarkable.

Thus, in this instance at least, we see that this horn is of direct use in aiding the insect to reach its food.

R. W. Doane

Stanford University,
September, 1913

SCIENCE AND THE NEWSPAPER

While recently giving a discussion of the inclined plane, an idea which was new to me suddenly presented itself. The equation asserts that the force required to make a mass slide up the plane would under certain conditions be made less, by making the plane

steeper. A student reporter thought it to be his duty to announce to the newspaper world that a new law of physics had been discovered, and the importance of the discovery seems to have increased with each successive announcement.

This experience reminds me of a similar one which happened to me years ago. At the time when reporters everywhere were rushing to physics laboratories in order to learn something of X-rays, a reporter came to me. He found me experimenting with Hertz waves. By means of a large double-convex lens of wax, the waves were being brought to a focus upon a photographic plate enclosed in a wrapping of black paper. For several weeks I had been trying to produce a shadow picture upon the plate. The reporter seemed interested, and he seemed to have some intelligence. He could appreciate the evidence that the lens caused a refraction of the rays. Although he was informed in the most emphatic manner that this was not a refraction of X-rays, the public announcment was made that I had succeeded where others had failed, in the refraction of X-rays.

It seems to be impossible to quench a disturbance of this kind when it has once been emitted from a news-agency. Scientific readers have probably had enough of such experience to see the importance of keeping, in an accessible place, a few grains of salt.

Francis E. Nipher

THE INDUSTRIAL FELLOWSHIPS AT PITTSBURGH

To the Editor of Science: The industrial fellowship project, originated in the University of Kansas by Professor Robert K. Duncan and now in flourishing operation under his direction in the University of Pittsburgh under the name of the "Mellon Institute of Industrial Research and School of Specific Industries," has been more than once subjected to the criticism which found a place in an otherwise favorable reference in the presidential address of Mr. Arthur D. Little to the American Chemical Society at its recent meeting at Rochester:[1]

> *While some doubt may reasonably be expressed as to the possibility of close individual supervision of so many widely varying projects,* the results obtained thus far seem entirely satisfactory to those behind the movement.

When first made this criticism had, I think, some validity. But to any one who has come into touch with the Mellon Institute, even as a visitor, it must be evident that the difficulty has been squarely met by "those behind the movement." The endowment of the fellowships is now so liberal as to permit of the employment of investigators of experience, who do not require "close individual supervision." In consequence, the relations of the Director and the Fellows are rather comparable to those of a university president and his corps of professors and instructors than to those of a university professor and his class of graduate students. Furthermore, the director is now assisted in the work of supervision by an associate director and an assistant director. Thus the services of three advisers are at the command of each Fellow, who may, moreover, obtain help from his colleagues without divulging the secrets of his own research.

If one acquainted with the project merely as an onlooker might venture an opinion upon the qualifications most essential to the success of the director of such an institute, it would be that a wide and sound general knowledge of scientific principles, a broad sympathy enabling one to appreciate the widely differing viewpoints of business men and of investigators and inventors, an active but disciplined scientific imagination and a strong, firm will are of more importance than an encyclopedic acquaintance with details.

J. F. Snell

Macdonald College
Quebec, Canada,
November 18, 1913

[1] Science, November 7, 1913, p. 652.

SCIENTIFIC BOOKS

Untersuchungen ueber Chlorophyll. Methoden und Ergebnisse von Richard Willstaetter und Arthur Stoll. Ein Bd., pp. 424, mit 16 Text-figuren und 11 Tafeln. Verlag von Julius Springer, Berlin. 1913. M. 18.00, geb M. 20.50.

If the well-known saying of Goethe "Denn eben wo es an Begriffen fehlt, da stellt ein Wort zur rechten Zeit sich ein" applied in the past to any group of phytochemical substances, its application to plant pigments was certainly justifiable. Such designations as "the green coloring matter of leaves," or "the blue coloring matter of flowers" are not as euphonious as chlorophyll and anthocyanin, but it is doubtful if they would have done as much harm. These words of Greek origin certainly enjoyed the advantage of brevity as well as of euphony, but they also carried with them something of a notion that they stood for more or less definite chemical compounds about which we flattered ourselves that we knew something, although this knowledge had not crystallized into structural formulas, the chemical shorthand expression of their properties. Plant physiologists were not the only sinners in this direction, but chemical literature is almost equally replete with illustrations of such misleading use.

To any one who is at all acquainted with the chemical literature on plant pigments, the researches of Willstaetter and his colaborers, as they have made their appearance in the *Annalen* since 1906, have come as a great relief. It is equally a relief, though of a different kind, to have the results, as laid down in these twenty-two Abhandlungen, together with more recent ones, coordinated to a "gemeinsames Ganzes." If we have admired Willstaetter's experimental researches, we are more grateful for his literary labors that have made available to us the results of his labors in the laboratory.

Even a partial review of the contents of this monograph would lead too far for a nontechnical journal like SCIENCE. Suffice it to point out that all aspects of the subject, it would seem, are treated in such a manner that the person who desires to inform himself in a general way can use the book to advantage as well as the investigator who is particularly interested in this special field. Plant physiologists as well as chemists will find the volume replete with useful information as well as interest.

We have here another illustration of German "Gruendlichkeit" that is not impaired by specialization and detail, but that has accomplished the best because of special effort on the one hand and because of the application of a wide general knowledge to a restricted problem on the other hand. It reminds one of Berzelius's letter to Woehler in which the older Swedish chemist pats his young German friend on the back, as it were, when, in words that one would scarcely look for to a chemist, he makes light of the more or less accidental discovery of a new element by Sefstroem—a discovery that had just escaped Woehler—as compared with the brilliant and far-reaching researches of the man to whom is commonly attributed the first organic "synthesis."

If the Germans have felt the necessity of supplementing the research activities, that have so long been characteristic of the scientific institutes of their universities, by the Kaiser Wilhelm Foundation, this contribution from the "Kaiser Wilhelm-Institut fuer Chemie" may well serve as a good omen of the excellent results that may be expected in the future from this new institution devoted to scientific research.

If the knowledge that we now have to deal with definite chemical substances when we speak of the "Abbau" products of chlorophyll and its partial synthesis, affords a feeling of satisfaction, the excellent microphotographic views of the crystals of these substances assist in strengthening the feeling that our present knowledge, as elucidated by Willstaetter, rests on a good foundation. E. K.

The Principles of Stock-breeding. By JAMES WILSON, M.A., B.Sc., Professor of Agriculture in the Royal College of Science for Ireland, Dublin, author of "The Evolution of British Cattle and the Fashioning of Breeds." Published in 1912 by Vinton and Company, Ltd., 8 Bream's Buildings, Chancery Lane, E. C., London. 8vo. Pp. vi + 146.

This book is an exposition of the recently

discovered principles of heredity, and an attempt to demonstrate their utility in practical stock-breeding operations, with especial reference to the economic production of milk and butter. In the first chapters Professor Wilson develops, in a manner that should interest both the student of heredity and the practical breeder, the history of the theory of stock-breeding, beginning with the old theories, which he designates: "like begets like," "inbreeding," "pedigree" and "evolution." Concerning these theories he says, "They have been tried in Britain for varying periods of time: like begets like for centuries, inbreeding for nearly a century and a half, and pedigree for nearly a century. Evolution has been in stock-breeders' minds vaguely for nearly a half century." He describes the rise of each of these notions, and tells how each in turn was adopted by the practical breeders and how each in turn was found to possess exceptions and shortcomings which the breeder was bound to recognize. He then points out the manner in which the aggravating exceptions to these accepted principles led to further investigations, and finally to the discovery of other principles at first accepted all too inclusively, only to be subjected to the same purifying process.

The history of the making of the breeds of British cattle is always a fascinating story, and Professor Wilson, through his wide acquaintance with the history of breeding, describes the inestimable service rendered to live-stock interests through the operations, largely by the process of inbreeding, first of all by Bakewell with many breeds, then by Hugh Watson with Angus cattle, and Cruickshank with Shorthorns, and by Sir George Macpherson Grant with Aberdeen-Angus cattle. The greatness of the English breeders is demonstrated by their willingness to try out all theories that promised utility. They threshed out the grain from the chaff; not only did they try out the old theories just mentioned, but they tried out with equal avidity "reversion," "maternal impression," "accident and mutilation" and "telegony." The fact that these latter theories yielded no "fruit" did not daunt the British breeder, and he is now in the midst of trying out Mendelism. If the principles of Mendelism, when applied to practical breeding, can yield half as much as the older inbreeding operations, then Professor Wilson's appeal and advice will prove to have been wholesome and good.

There is in this book a vigorous protest against pedigree breeding in the old sense, and a continual appeal for breeding for traits which can be controlled by the applications of Mendelian principles. The author contends that the herd-books and stud-books are the tyrants that keep modern breeds stationary; that fashion, as much as utility, seems to rule the older breeds, the one exception being the thoroughbred horse, which is continually being put to the best of tests, namely, the track, and winners and breeders of winners are in demand regardless of family tradition. He prophesies that one of the principal lines of development of stock-breeding in the future will be the transferring of traits of utility from one breed to another, and is optimistic as to the possibilities of such a process.

The author describes the instances wherein traits of domestic animals appear to behave in Mendelian fashion, and he attempts to give practical advice as to the proper method of breeding for what he is pleased to call the three economic factors, namely, size, yield and quality.

In reference to the first, size, it appears that the first cross between cattle of a small and a large breed will give, quite uniformly, an intermediate-sized animal, but it is not clear whether such animals when bred together will throw offspring which segregate back to the two grandparental sizes. He protests against the method of breeding the half-breed offspring back to one of the pure breeds, claiming, quite properly it appears, that the correct way to secure new combinations is to breed the F_1 hybrids together. He protests also against too close an adherence to the theory of fancy points, holding that there is not always the high correlation between fashionable points and utility that many breeders seem to feel exists.

In discussing the second factor, the quality of milk yield, the author describes an experiment conducted by Count Ahlefeldt, wherein Red Danish cattle, with an average yield of 3.42 per cent. milk, were crossed with Jerseys averaging a yield of 5.22 per cent. milk. The hybrid offspring averaged a yield of 4.15 per cent. These cross-bred animals were bred back to the parental Jerseys. The author points out that if quality of yield behaves in Mendelian fashion, one half of the animals, regardless of their other traits, would yield milk of Jersey quality, and one half of them would yield the cross-breed quality. Analyzing the table given by the author, we find that of the 15 offspring of such matings 7 yielded 4.7 per cent. or richer milk, and 8 yielded below this quality. If the types of offspring from the *Cross* by *Red Danish,* and *Cross* by *Cross* matings approximate as closely to the Mendelian expectation as the *Cross* by *Jersey* mating just described, and the matings are extensively made, then, even though yield may be governed by a host of unit traits, they would appear, for practical purposes, to move in synchronism, and the practical breeder would have a working principle of value. One would suspect, however, that such a complex thing as quality would shatter in the subsequent inbreeding of hybrids. More data are required.

The author points out that yield of butter is not a fair basis for breeding selection, because butter yield is dependent upon two factors, namely, quality and quantity of milk. Each one of these factors should be taken as a basis for selection, and a combination of high quality and high yield sought by Mendelian methods. He sees no sound reason why high quality and great quantity of yield should be mutually exclusive; he believes they can be combined by Mendelizing.

If any adverse criticism were to be rendered, it must be said that throughout the book the author disregards the exceptions to the rule when describing the heredity of an animal characteristic which appears to approximate Mendelian expectation. For instance, continual reference is made to color inheritance in Shorthorn cattle, assuming the case exactly parallel to that of the Andalusian fowl, wherein the first generation hybrid is a blend and segregation occurs in the second generation according to Mendelian formula. Whereas it has been found that Shorthorn coat color is neither one unit nor a single group of units, but behaves in heredity as two units, or unit groups, the areas for the white hairs in the roan behaving as one unit, and the areas for the red as another. Moreover, a red mated with a red does not *always* produce a red, although it *generally* does so. If the whole coat color were a single unit, behaving in Mendelian fashion, then *red* by *red* would produce only red. To a well-known exception of this sort the author should not be blind; to him, as he so clearly points out in reference to the older studies and theories, it should point toward future studies and discoveries, each with its gold and dross. It would seem more reasonable continually to urge the analysis of gross somatic characteristics into heritable units which, without exception, behave according to rule. However, a rule that works nine times out of ten is a good one for the practical man to follow, and to him is an instrument of inestimable value, although to the theorist the one exception is the thing that commands his interest and work.

To summarize, the book is a special plea for the practical application of the Mendelian principles to animal breeding, and as such, the case is better established than in any other practical breeder's guide with which the reviewer is acquainted. In general, it recognizes the limitations of the present knowledge of Mendelian traits in domestic animals, and in a wholesome manner urges further investigation, as well as the courageous application of current theories by practical breeders.

The author's style is literary, his English clear, and his argument is easy to follow.

H. H. LAUGHLIN
EUGENICS RECORD OFFICE,
COLD SPRING HARBOR, LONG ISLAND

The First Principles of Evolution. By S. HERBERT. London, A. & C. Black; New York, The Macmillan Co. 1913.

Notwithstanding the large number of books that have already been published on evolution, the author of the above work believes that there is still a need for another which will present the subject, not as a theory that is on trial, but as an established principle in terms of which men must be taught to think. The popular tendency to regard evolution and Darwinism as synonymous terms is the result of the historical development of the theory largely on the basis of facts derived from organic nature, and its wider application as a philosophical principle has been thereby obscured. To correct this misconception the earlier chapters of the present work are devoted to an exposition of cosmic, geological and atomic evolution, this last leading to a brief and rather inadequate consideration of the origin of life, whence there is a natural transition to the discussion of organic evolution. Unfortunately, however, for the broader conception which the author seeks to emphasize, this last and more familiar side of the subject is given more than three times the amount of space granted inorganic evolution and this is all the more regrettable since the treatment of organic evolution does not compare altogether favorably with that to be found in other familiar works which naturally suggest themselves, especially since the illustrations are merely reproductions of well-known figures from Darwin, Wallace, Weismann and especially Romanes. Credit must be given, however, for a clear and concise statement of the various theories that have been advanced as an explanation for organic evolution, Darwinism and Neo-Darwinism, Lamarckism and Neo-Lamarckism, mutations, orthogenesis, entelechies, Bathmism and even the metaphysical subtleties of Bergson being briefly expounded and criticized.

The last hundred pages of the book are devoted to what the author terms superorganic evolution, under which heading are discussed mental, moral and social evolution, sufficient being said upon each of these topics to give the reader a fair idea of the trend of modern thought in connection with questions of the utmost importance to society.

The book is one that may be sincerely recommended. Like an earlier work by Dr. Herbert, "The First Principles of Heredity," it is the outcome of a series of lectures delivered to popular audiences, and, while clear and concise in statement, it is excellent reading. A well-selected bibliography is appended and also a glossary of unavoidable technical terms.

J. P. McM.

SPECIAL ARTICLES

ON FUNDAMENTAL METHODS OF ORIENTATION AND "IMAGINARY MAPS"

The following paper presents a study of the reasons why civilized man is so apt to lose his bearings in unfamiliar regions. This question of orientation apparently has been neglected heretofore.

In an investigation of the "sense of direction" or the "sense of locality," it is important to classify the fundamental methods of orientation employed by living creatures. There appear to be two radically different methods; one used by civilized man, the other chiefly by living creatures of a lower order. The former, which employs the points of the compass, is acquired artificially by education. It is proposed to call this the *ego-centric method.* The latter is used not only by birds, beasts, fish, insects, etc., but also, in all probability, by young children and by a large proportion of mankind living in an uncivilized state. In this system of orientation the points of the compass play little, if any, part, and it may be designated as the *domi-centric method.* The selection of these terms by the author will be explained below.

The Ego-centric Method of Orientation.—Civilized man, by artificial training, has become accustomed to orient himself by the four points of the compass: north, east, south and west; and indeed wherever he may be, he usually finds his way by this method, except in the neighborhood of his dwelling place. In the immediate vicinity of the home the orientation nearly always relates to the home as a center of reference, irrespective of the points of the compass, and in this limited region the

method of orientation is largely domi-centric.

The orientation reference points in the *ego-centric method* are points on the horizon corresponding to the directions N., E., S. and W. Lines from these points always intersect at the ego, the intersection moving with the ego; hence the basis for the term given to this system of orientation.

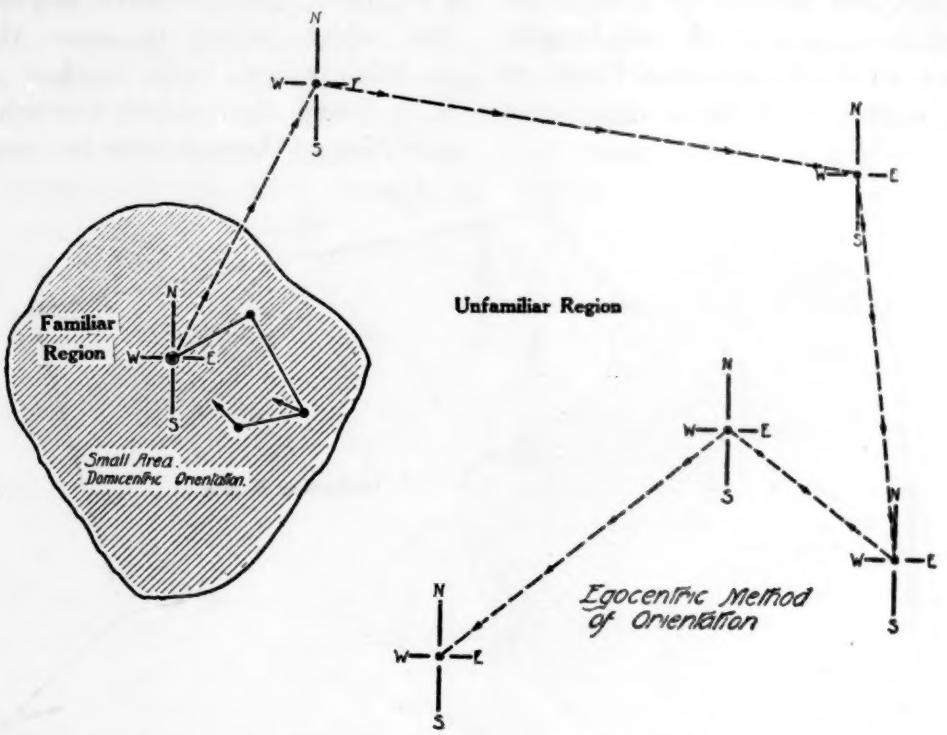

Fig. 1. Ego-centric Method of Orientation. In the unfamiliar region the reference points are objects or points on the horizon corresponding to the direction N., E., S. or W.

It is, of course, well known that when a man is wandering through any maze-like region, such as a primeval forest, the compass gives the direction from the man toward the north, or more strictly, the north magnetic pole, and to all other parts of the compass, but not the direction to the man's starting point; thus the ego-centric method is not a system *per se* which will direct the individual to his home. This system of orientation, therefore, (*a*) leads man to think of space in relation to the cardinal points of the compass; (*b*) it can be used to direct an individual home only when the path which he has passed over is known. The method is illustrated by Fig. 1.

The Domi-Centric Method of Orientation.—All living creatures, other than civilized man, having no knowledge of the points of the compass as such, or of the extent of the world, know only the region which they have traversed. Thus it follows that from the time these creatures come into existence their movements, instead of being referred to points of the compass, relate to the place where they began their existence, and hence in early life their knowledge of space must necessarily be related to the place of birth. This system of orientation, centering at the home and irrespective of the points of compass, has been called the *domi-centric method*, and is illustrated by Fig. 2. The Esquimaux, Indians, etc., evidently have a method of orientation which is not definitely in any one class, but is rather a combination of the two methods already mentioned.

If the home of any animal is changed for a considerable period of time to a region away from its former habitation, thenceforth all movements will be referred to the last principal reference point, or home. In this case the *domi-center* has changed.

It is well here to emphasize the entirely different mental concept of civilized human beings, on the one hand, and of other living

creatures, on the other, relating to space on the earth's surface. The former look outward towards the horizon, the latter look backward toward their starting point. To the first no opportunity is offered for expertness through experience, to the second is given an opportunity for a reflex mechanism. In the ego-centric method, it is as if the man were attached to the four cardinal points of the compass by elastic threads of indefinite lengths, which present no basis whatever (lines or angles) for a trigonometric figure that relates to the home.

responsible at times for man's confusion when attempting to find his way, as will be shown. In the other, the domi-centric system of orientation, experience continually leads an animal to greater expertness in finding its way home, and the conditions are present for a reflex mechanism.

The Imaginary Orientation Map.—There is a feature of the *ego-centric method* of orientation which seems to show that the use of this system leads to loss of bearings. It is found that either through loose early education or through later impressions persons

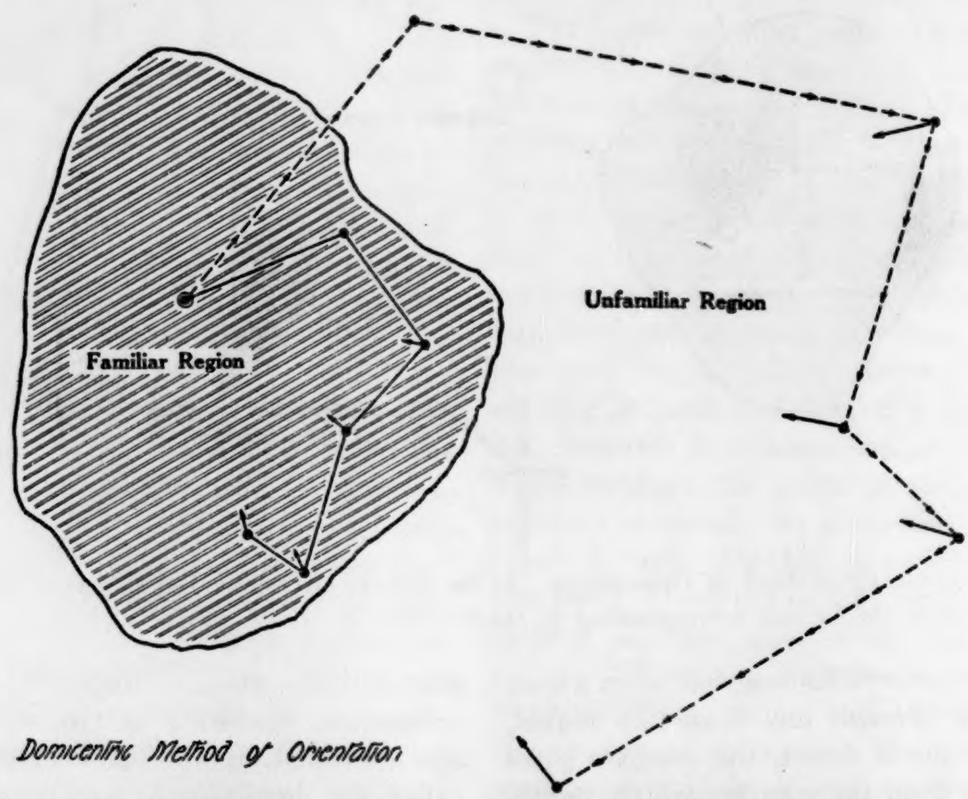

FIG. 2. Domi-centric Method of Orientation. The chief reference point is always the starting point, or home. Around about are usually many minor reference points; familiar objects which give a definite reaction relating to the home.

In the case of insects, birds, mammals, etc., which orient themselves domi-centrically, it is as if the living creature were attached to its home by one very strong elastic thread of *definite* length. Hence, in this case, all changes of position of the creatures can be referred at any moment, to definite distances and angles, forming a simple trigonometric figure which gives the direction to the home.

In the two types of orientation methods, the use of one, the ego-centric system, actually is

are apt to acquire erroneous ideas of the directions *toward very distant places of the earth,* frequently becoming gradually accustomed to think of the points of the compass which correspond to these distant places with a large error of direction, amounting in some cases to as much as 180°, or diametrically opposite to the correct location. This leads to the conception of a mental image of an orientation map that is entirely imaginary, and erroneous. This imaginary orientation map appears to be

similar to, if not actually connected with, punctuation, the visualization process. It will be found by questioning various individuals, that the orientation of many persons for very far distant points, as they usually think of these places, is in error to the extent of 30°, 90° or even 180° (or half circle). Fig. 3 is a diagram drawn to illustrate what is meant by an "imaginary map." In this figure the solid lines represent the map as it actually is. The dotted lines represent the map as the subject is accustomed to think of it. An important fact in this connection has been found, namely, that those individuals who have "imaginary maps," are readily confused in regard to locality, are apt to become lost in the forests, and usually are subject to confusion as to direction when emerging from theaters, subways, etc. On the other hand, those individuals who through careful early education or from travel are accustomed to *think* of far distant places in the proper directions, are much less apt to be confused in regard to locality. This is readily evident from the accompanying statistics. An extensive analysis of the precise forms of the erroneous conceptions with respect to the direction toward distant places has not been attempted. A few of the more common types will be given which will help to emphasize the fact that this so-called imaginary map which accompanies the "ego-centric" or cardinal point method of orientation unquestionably contributes to the difficulty that man experiences in finding his way home in an unfamiliar region.

Various Types of Imaginary Maps.—The common types are described below. A complete classification would be difficult since the types must grade into one another, but most of those mentioned appear to be common forms.

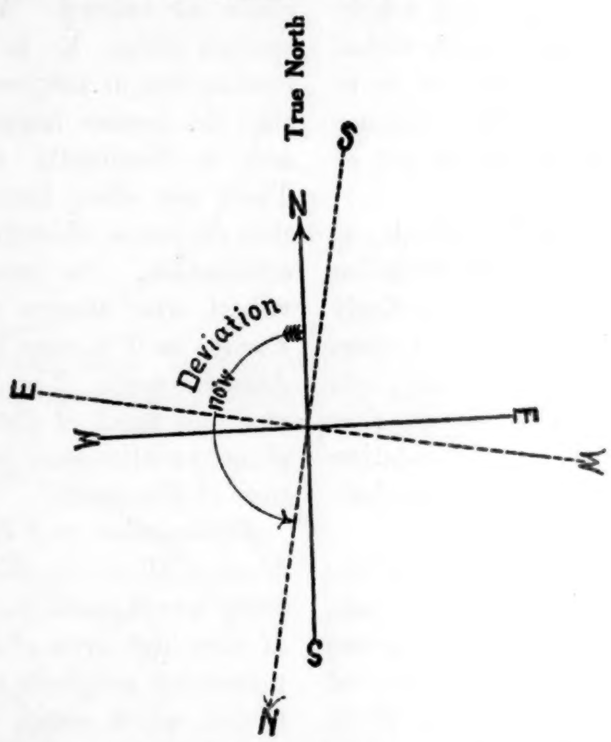

FIG. 3. Diagram to Explain the Imaginary Orientation Map. The solid lines indicate the points of the compass, and is the map which the subject knows to be correct. The dotted lines indicate the map as the subject is accustomed to think of it when far distant places are casually thought of.

The types will be classed as individuals.

Type A.—Those persons who have an "imaginary map" of fairly consistent "deviation" from the correct direction for the entire circuit of the compass. (A common type.) The "deviation" refers to very distant places, and in this class amounts to from 20° to 180°. It is the angle between the true directions of distant places and the directions that the subject casually thinks these places lie in.

Type B.—Those who have different "imaginary maps" in different localities. The following example of an actual case will illustrate

this type, which should include different parts of a large city as well as different localities in the country.

The map of E. F. H. represents a noteworthy case of Type *B*, but probably not uncommon. His average deviation (for distant places) at 116th Street in the City of New York is 156° west, the average variation of the mean of one set of observations of four distant places being only 5°. At 42d Street in the same city, his imaginary map is about 90° wrong, that is, the deviation is 90°, and at 14th Street the imaginary map disappears. Likewise his orientation is 90° wrong at Toronto, Canada, correct at Chicago, and nearly correct in country districts away from cities. Mr. E. F. H. is almost always confused as to the direction toward his home when coming out of theaters and often when coming out of subways.

Type C.—Those who imagine north as directly in front of them. Thus the deviation of the imaginary map is determined entirely by the direction in which they may be facing, the east being at the right hand, the west left hand. The imaginary map is consistent, that is, all places have nearly the correct relation to the north, and turns with the subject. (Common type.)

Type D.—Those to whom *all distant* points lie either toward the west or toward the east. For example *both* Madrid and San Francisco appear to lie to the west from an individual of this class residing in New York City. (Two well-defined cases.)

Type E.—Those who think of far distant points in approximately the correct direction, but to whom distant countries appear rotated. For example, to one individual while England appears in approximately the correct direction from New York, the entire British Islands are rotated about 180°; both the English Channel and France appearing to lie to the north of the British Isles. (One case.)

Type F.—Those who have an imaginary map that differs consistently about 20–40 degrees from the correct one, apparently due to the influence of the direction of certain rivers and streets which for one reason and another have had a marked orientation influence on the subject. (Several cases.) It is possible that this is the same as type *A*, yet the cause of the confusion appears to be different.

Type G.—Those having an imaginary map that always makes certain streets in every city exactly north and south, others exactly east and west, with all diagonal streets 45°, as if lying northeast and southwest, or northwest and southeast. (Several cases.)

Another type is that of a person who has had an imaginary map, but who has gradually overcome it by education. In one case the subject had an imaginary map for four years while at college. At the present time in various cities, he is usually confused when coming out of theaters, etc., and it is possible that the former imaginary map is still latent and is frequently a source of confusion. There are other features of imaginary maps that do not so directly bear on the question of orientation. For example, there is one individual who always thinks of, or visualizes Europe as if it were but 20 to 40 miles off the Atlantic coast. Then, of course, the majority of people think of distant places as points on a plane, no allowance being made for the curvature of the earth.

Explanation and Importance of Imaginary Maps.—All of the above types, *A–G*, are taken from actual cases, the subjects being as a rule of very high type of intellectuality, university professors, graduate students, etc. The explanation which seems to be the most plausible one to account for this so-called "imaginary map," is the persistence of early erroneous impressions concerning the direction of far distant places with respect to the home, the mistaken ideas arising from various causes. These impressions apparently take a firm hold during childhood. The matter is of some importance, since it accounts in a measure for the readiness of man to be confused with respect to a new environment, and to become "lost" in the woods or in any maze-like surrounding. An example of a practical bearing is as follows: The matter has a pertinent relation to the training of children who are to become soldiers, especially in countries where standing armies are maintained. In times of war, it is not im-

probable that the loss of more than one battle has been due to the utter confusion of officers or of small bodies of troops with respect to points of compass, due to the concentration of attention on the enemy in the height of action or during maneuvers at night.

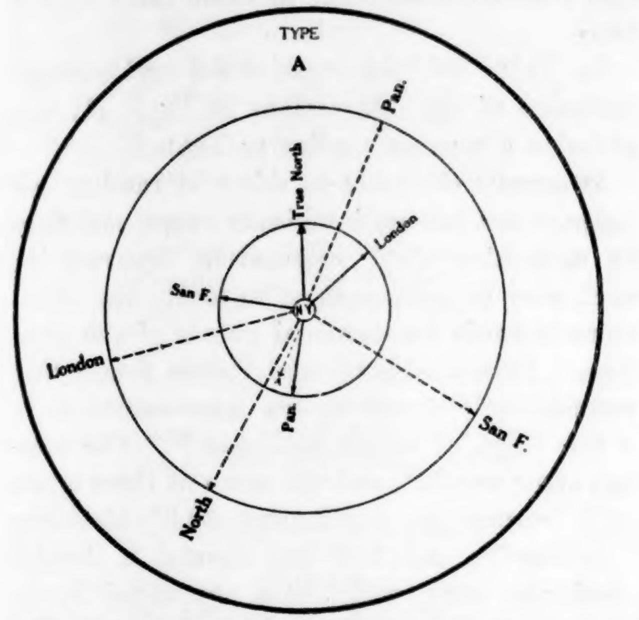

FIG. 4. Type A. Imaginary Map. The amount of deviation is the same amount under all conditions, and in all places.

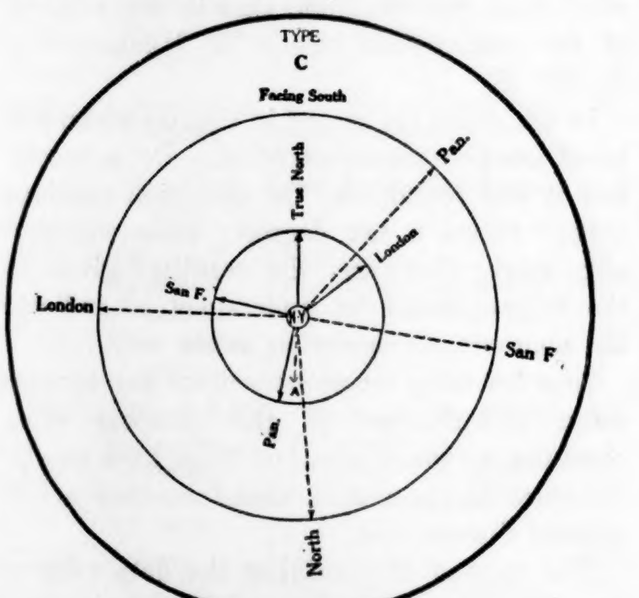

FIG. 6. Type C. Imaginary Map. The map depends on which way the subject is facing.

If it is desirable to correct this very common defect in orientation training, it would appear necessary that children should be seated at school in a special manner when studying geography, with the cardinal points of the compass marked in the room, and the maps in the books properly orientated, and the imaginary maps systematically corrected in childhood.

The proportion of people who have so-called "imaginary maps" is astonishingly large,

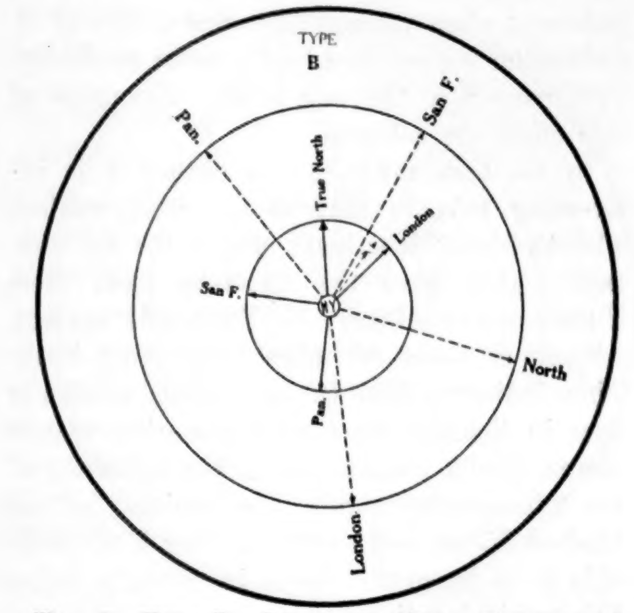

FIG. 5. Type B. Imaginary Map. The amount of deviation may vary with the place in which the subject happens to be.

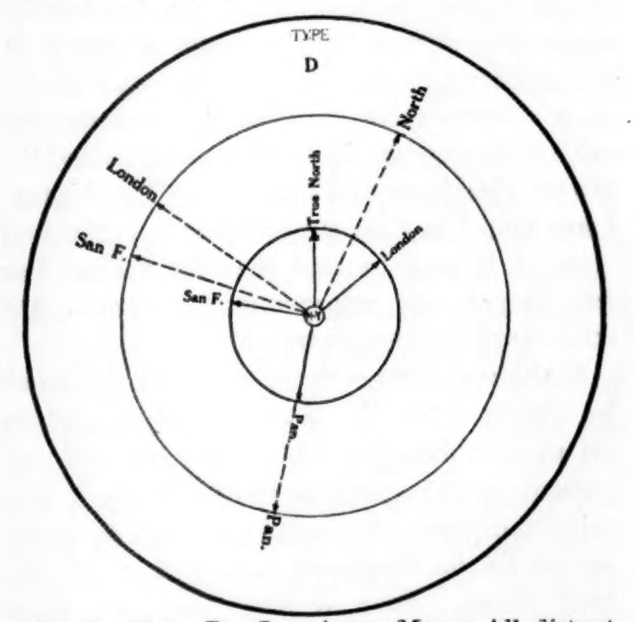

FIG. 7. Type D. Imaginary Map. All distant places appear to be west (or east) of north.

being of the order of thirty to fifty per cent., if not a much higher ratio; hence the matter is one of general interest.

The object of the presentation of these facts

is to show that children of civilized parents, through accidental faults in early education arising from the faculty of vivid imagination, and owing to the misuse of the "*ego-centric*" or cardinal point method of orientation, build up persistent impressions quite erroneous, which later on in life unconsciously affect their judgment when attempting to find their way in unfamiliar regions and lead to utter confusion with respect to the way home. Examples of this effect are common.

In the tests made by the author it is interesting to note that almost every subject who had an "imaginary map" for far distant places, gave the direction from New York towards Albany, N. Y., nearly correct. Albany is about 90 miles from New York. This indicates that the education gradually fixes in thought the correct direction toward places, finally overshadowing the influence of the "imaginary map." The position of the Hudson River with respect to New York probably is an important factor in correctly fixing this particular direction.

It must be distinctly understood that the directions in "imaginary maps" are not as the subject knows the directions to be, but merely where they always imagine them as being in the ordinary process of thinking, and *in all cases referred to in the present discussion the subject having an imaginary map, knew the correct directions approximately.* The "imaginary map" is thus superimposed on the real map, or it may be said that the subject has two maps; one approximately correct, the other entirely imaginary.

Statistical Data.—Some statistics are given in Tables I. to V. The subjects on whose orientation data the tables are based were all persons of university training. Table I. contains ten cases of imaginary maps as determined by the directions towards four far distant places. Four of these maps are given diagrammatically in Figs. 4, 5, 6 and 7, representing different types which have been classified as *A, B, C* and *D*, respectively. In Table II. is given the mean error and average variation of the observations recorded in Table I.

Table III. contains ten cases of subjects having no imaginary maps and includes the angular deviation from the correct directions. It is seen that there may be very large errors in some cases in locating the direction towards the places selected for the test. All the subjects in Table III. had but one orientation map, however, while those in Table I. have two.

In Table IV. the mean error and average variation of the observations in Table III. are given in a manner similar to Table II.

It seemed desirable to select at random ten subjects not having imaginary maps, and then to determine their orientation accuracy in each case by asking them to locate the directions towards the cardinal points of the compass. This was done, and it was found that astonishingly large errors were recorded in a few cases, as shown in Table V. The average error was 30°, and the mean of these errors with respect to north was 22.6° clockwise (eastward). All but two showed a decided clockwise error, which was accounted for by reason of the prevailing idea that the chief avenues in New York lie approximately north and south. Actually they lie 29 degrees (clockwise) from the meridian, that is, the azimuth of the longitudinal streets of Manhattan is N. 29° E.

In the tables the record in degrees given was based on but one observation. By a special test it was found that the deviation readings always varied a few degrees; some considerably more; therefore, the readings given in the tables should be understood to indicate the approximate deviation angle only.

In a few cases errors were made due to magnetic disturbances of the compass when checking up the charts, but these have no significance in the article, therefore, they are at present disregarded.

The method of obtaining the data relating to "imaginary maps" was as follows: A circular piece of paper was placed before a subject, who was requested to mark on the disk the directions from the center of the disk, New York, N. Y., to the North Pole, London, San Francisco and Panama, as these places appeared to him. The magnetic north was then

TABLE I

Deviation of Subjects Having Imaginary Maps

Name	Place Located	Deviation from Correct Location	Type
J. C. H.[1]..	North Pole	154° counter-clockwise	*A*
	London	70° counter-clockwise	
	Panama	86° counter-clockwise	
	San Francisco	31° counter-clockwise	
J. M......	North Pole	110° clockwise	*A*
	London	126° clockwise	
	Panama	134° clockwise	
	San Francisco	111° clockwise	
C. G. S.[2]..	North Pole	42° clockwise	*A*
	London	82° clockwise	
	Panama	29° clockwise	
	San Francisco	21° clockwise	
C. C. T.[1]..	North Pole	138° counter-clockwise	*A*
	London	126° counter-clockwise	
	Panama	134° counter-clockwise	
	San Francisco	150° counter-clockwise	
R. C.[1]......	North Pole	117° clockwise	*A*
	London	156° clockwise	
	Panama	121° clockwise	
	San Francisco	107° clockwise	
B. R. R.[1]..	North Pole	79° clockwise	*A*
	London	117° clockwise	
	Panama	108° clockwise	
	San Francisco	78° clockwise	
E. F. H.[1].	North Pole	149° counter-clockwise	*B*
	London	153° counter-clockwise	
	Panama	165° counter-clockwise	
	San Francisco	157° counter-clockwise	
W. A. H.[1]	North Pole	175° clockwise	*C*
	London	139° counter-clockwise	
	Panama	149° counter-clockwise	
	San Francisco	177° clockwise	
P. C.[1]......	North Pole	49° counter-clockwise	*C*
	London	11° clockwise	
	Panama	93° counter-clockwise	
	San Francisco	124° counter-clockwise	
J. D.[1]......	North Pole	26° clockwise	*D*
	London	106° counter-clockwise	
	Panama	1° clockwise	
	San Francisco	8° clockwise	
R. R.[1]......	North Pole	21° clockwise	*H* (perhaps type *D*)
	London	151° clockwise	
	Panama	60° clockwise	
	San Francisco	26° clockwise	

obtained by a compass and marked on the disk. The true north was ascertained later.

The correct direction from New York, N. Y., to the distant points above mentioned was obtained from one of the staff of the American Geographical Society who made the necessary calculations. They were as follows:

North Pole	0° 0′	
London	51° 10′	(51° 10′ east of north).
Panama	190° 20′	(10° 20′ west of south).
San Francisco ...	281° 25′	(78° 35′ west of north).
Albany, N. Y. ..	4° 59′	(4° 59′ east of north).

The percentage of individuals having the so-called imaginary map can only be decided by extensive data on the subject, but in order to learn the approximate ratio in a certain class, twenty-seven persons, taken at random, were questioned. The results were as follows:

Total number of persons (males) consulted....	27
Those having "imaginary maps"............	16
Those having no "imaginary maps"..........	8
Cases that were uncertain....................	3

Of the 16 having "imaginary maps" 14 were more or less confused when coming out of theaters, subways, etc.

Of the 8 having no "imaginary maps," 7 were not confused when coming from theater and had in general a good "sense of direction." (These ratios are similar to those in Tables I. and III.).

According to these figures, the number of persons in 27 having "imaginary maps" was about 59 per cent. These statistics are far too few on which to base any general conclusions other than the prevalence and importance of this curious so-called "imaginary map."

Certain physiological effects connected with this matter are of interest; Yves Delage has touched upon the subject in his "Essay on the Constitution of Ideas." He states that when he is "turned around" or confused in regard to direction, he feels a sensation of illness at the moment of rectification of his notions.

1 Subject is confused as to directions on coming out of theaters and subways.

2 Subject is not usually confused as to directions on coming out of theaters and subways.

Henri de Varigny in the "Revue des Sciences" of the *Journal des Débats* (Paris, April 17, 1913), discussing the above essay, states that under the same circumstances he has an impression like a slight vertigo, the feeling being localized clearly at the base of the skull.

The work in this investigation has been aided by a grant by the New York Academy of Sciences from the Esther Herman Fund.

TABLE II

Average Error and Variation in the Case of those Subjects Having Imaginary Maps

Name	Mean Error of Four Places Located	Average Variation from Mean	Type
J. C. H., Jr..	85° counter-clockwise	35°	*A*
J. M............	120° clockwise	10°	*A*
C. G. S........	44° clockwise	20°	*A*
C. C. T........	137° counter-clockwise	7°	*A*
R. C., Jr......	125° clockwise	15°	*A*
B. R. R........	96° clockwise	17°	*A*
E. F. H........	156° counter-clockwise	5°	*B*
W. A. H.......	160° [3]	16°	*C*
P. C............	69° [3]	39°	*C*
J. D............	35° [3]	35°	*D*
R. R............	64° clockwise	43°	*H*

Column 2 gives the average angle between the true directions of distant places and the directions in which the subject thinks these places lie.

Column 3 indicates the inconstancy of this angular displacement or deviation.

TABLE III

Deviation of Subjects who have No Imaginary Maps

Name	Place Located	Deviation from Correct Location	
H. C.	North Pole	?	(chart confused).
	London	5°	clockwise.
	Panama	11°	clockwise.
	San Francisco	23°	counter-clockwise.
E. L. K.[4] ..	North Pole	0°	
	London	21°	clockwise.
	Panama	10°	counter-clockwise.
	San Francisco	20°	counter-clockwise.
J. H. M.[4] ..	North Pole	6°	clockwise.
	London	31°	clockwise.
	Panama	18°	counter-clockwise.
	San Francisco	16°	counter-clockwise.
E. F. K.[4] ..	North Pole	14°	clockwise.
	London	43°	clockwise.
	Panama	4°	counter-clockwise.
	San Francisco	14°	clockwise.
W. H. G.[5] ..	North Pole	14°	clockwise.
	London	41°	clockwise.
	Panama	6°	counter-clockwise.
	San Francisco	14°	clockwise.
H. W. W.[4]	North Pole	28°	clockwise.
	London	32°	clockwise.
	Panama	26°	clockwise.
	San Francisco	18°	clockwise.
H. M. R.[4] ..	North Pole	8°	counter-clockwise.
	London	4°	clockwise.
	Panama	36°	counter-clockwise.
	San Francisco	31°	counter-clockwise.
F. B.[5]	North Pole	8°	clockwise.
	London	17°	clockwise.
	Panama	4°	clockwise.
	San Francisco	14°	counter-clockwise.
W. A. D...	North Pole	34°	clockwise.
	London	48°	clockwise.
	Panama	2°	counter-clockwise.
	San Francisco	16°	clockwise.
J. C. G.[4] ..	North Pole	4°	counter-clockwise.
	London	8°	clockwise.
	Panama	13°	counter-clockwise.
	San Francisco	20°	counter-clockwise.

[3] Some errors clockwise, others counter-clockwise. See Table I.

TABLE IV

Average Error and Variation in the Case of Those Subjects Having No Imaginary Maps

Name	Mean Error of Four Places Located	Average Variation from Mean
H. C.	13° [6]	7°
E. I. K..........	13° [6]	8°
J. H. M..........	18° [6]	7°
E. F. K..........	19° [6]	12°
W. H. G..........	19° [6]	11°
H. W. W.........	26° clockwise	4°
H. M. R..........	20° [6]	14°
F. B.	11° [6]	5°
W. A. D..........	25° [6]	16°
J. C. G..........	11° [6]	5°

[4] Subject is not usually confused as to directions on coming out of theaters and subways.

[5] Subject is confused as to directions on coming out of theaters and subways.

[6] Some errors clockwise, others counter-clockwise. See Table III.

TABLE V

Errors in Locating the Cardinal Points of the Compass in the Case of Subjects Having No Imaginary Maps

Name	Direction	Deviation from Correct Direction	Mean Deviation or Error
W. S. N.	North	5° clockwise	24°
	East	37° clockwise	
	South	34° clockwise	
	West	18° clockwise	
R. M.	North	31° counter-clockwise	31°
	East	34° counter-clockwise	
	South	29° counter-clockwise	
	West	31° counter-clockwise	
F. N. C.	North	8° counter-clockwise	7°
	East	8° counter-clockwise	
	South	7° counter-clockwise	
	West	3° counter-clockwise	
W. A. D.	North	34° clockwise	1°
	East	34° clockwise	
	South	31° clockwise	
	West	25° clockwise	
A. C. M.	North	25° clockwise	22°
	East	19° clockwise	
	South	22° clockwise	
	West	23° clockwise	
T. E. H.	North	12° clockwise	15°
	East	5° clockwise	
	South	19° clockwise	
	West	22° clockwise	
H. F. J.	North	19° clockwise	22°
	East	24° clockwise	
	South	20° clockwise	
	West	25° clockwise	
G. F. W	North	79° clockwise	85°
	East	84° clockwise	
	South	88° clockwise	
	West	88° clockwise	
J. M. G.	North	52° clockwise	56°
	East	57° clockwise	
	South	59° clockwise	
	West	57° clockwise	
W. W. R.	North	11° clockwise	9°
	East	14° clockwise	
	South	2° clockwise	
	West	10° clockwise	

C. C. TROWBRIDGE

COLUMBIA UNIVERSITY

THE CONVOCATION WEEK MEETING OF SCIENTIFIC SOCIETIES

THE American Association for the Advancement of Science and the national scientific societies named below will meet at Atlanta, Ga., during convocation week, beginning on December 29, 1913.

American Association for the Advancement of Science.—President, Professor Edmund B. Wilson, Columbia University; retiring president, Professor Edward C. Pickering, Harvard College Observatory; permanent secretary, Dr. L. O. Howard, Smithsonian Institution, Washington, D. C.; general secretary, Professor Harry W. Springsteen, Western Reserve University, Cleveland, Ohio; secretary of the council, Professor William A. Worsham, Jr., State College of Agriculture, Athens, Ga.

Section A—Mathematics and Astronomy.—Vice-president, Dr. Frank Schlesinger, Allegheny Observatory; secretary, Professor Forest R. Moulton, University of Chicago, Chicago, Ill.

Section B—Physics.—Vice-president, Professor Alfred D. Cole, Ohio State University; secretary, Dr. W. J. Humphreys, Mount Weather, Va.

Section C—Chemistry.—Vice-president, Dr. Carl L. Alsberg, Bureau of Chemistry; secretary, Dr. John Johnston, Geophysical Laboratory, Washington, D. C.

Section D—Mechanical Science and Engineering.—Vice-president, Dr. O. P. Hood, U. S. Bureau of Mines; secretary, Professor Arthur H. Blanchard, Columbia University, New York City.

Section E—Geology and Geography.—Vice-president, J. S. Diller, U. S. Geological Survey; secretary, Professor George F. Kay, University of Iowa.

Section F—Zoology.—Vice-president, Dr. Alfred G. Mayer, Carnegie Institution of Washington; secretary, Professor Herbert V. Neal, Tufts College, Mass.

Section G—Botany.—Vice-president, Professor Henry C. Cowles, University of Chicago; secretary, Professor W. J. V. Osterhout, Harvard University, Cambridge, Mass.

Section H—Anthropology and Psychology.—Vice-president, Professor Walter B. Pillsbury, University of Michigan; acting secretary, Dr. E. K. Strong, Jr., Columbia University, New York City.

Section I—Social and Economic Science.—Vice-president, Judson G. Wall, Tax Commissioner, New York City; secretary, Seymour C. Loomis, 69 Church St., New Haven, Conn.

Section K—Physiology and Experimental Medicine.—Vice-president, Professor Theodore Hough,

University of Virginia; secretary, Dr. Donald R. Hooker, Johns Hopkins Medical School, Baltimore, Md.

Section L—Education.—Vice-president, Dr. Philander P. Claxton, Commissioner of Education, Washington, D. C.; secretary, Dr. Stuart A. Courtis, Liggett School, Detroit, Mich.

The Astronomical and Astrophysical Society of America.—December 29–January 3. President, Professor E. C. Pickering, Harvard College Observatory; secretary, Professor Philip Fox, Dearborn Observatory, Evanston, Ill.

The American Physical Society.—December 29–January 3. President, Professor B. O. Peirce, Harvard University; secretary, Professor A. D. Cole, Ohio State University, Columbus, Ohio.

The American Federation of Teachers of the Mathematical and the Natural Sciences.—Between December 30. President, Professor C. R. Mann, University of Chicago; secretary, Dr. Wm. A. Hedrick, Washington, D. C.

The Entomological Society of America.—December 30–31. President, Dr. C. J. S. Bethune, Ontario Agricultural College; secretary, Professor Alexander D. MacGillivray, 603 West Michigan Ave., Urbana, Ill.

The American Association of Economic Entomologists.—December 31–January 2. President, Professor P. J. Parrott, Geneva, N. Y.; secretary, A. F. Burgess, Melrose Highlands, Mass.

The Botanical Society of America.—December 30–January 2. President, Professor D. H. Campbell, Stanford University; secretary, Dr. George T. Moore, Botanical Garden, St. Louis, Mo.

The American Phytopathological Society.—December 30–January 2. President, F. C. Stewart, Agricultural Experiment Station, Geneva, N. Y.; secretary, Dr. C. L. Shear, Department of Agriculture, Washington, D. C.

The American Microscopical Society.—December 30. Secretary, T. W. Galloway, James Millikin University, Decatur, Ill.

American Association of Official Horticultural Inspectors.—December 29. President, E. L. Worsham, Atlanta, Ga.; secretary, J. G. Saunders, Madison, Wis.

The Southern Society for Philosophy and Psychology.—December 31–January 1. President, Professor H. J. Pearce, Gainesville, Ga.; secretary, Professor W. C. Ruediger, George Washington University, Washington, D. C.

The Sigma Xi Convention.—December 30. President, Professor J. McKeen Cattell, Columbia University; recording secretary, Professor Dayton C. Miller, Case School of Applied Science, Cleveland, Ohio.

Gamma Alpha Graduate Scientific Fraternity.—December 30. President, Professor J. I. Tracey, Yale University; secretary, Professor H. E. Howe, Randolph-Macon College, Ashland, Va.

PHILADELPHIA

The American Society of Naturalists.—December 31. President, Professor Ross G. Harrison, Yale University; secretary, Dr. Bradley M. Davis, University of Pennsylvania, Philadelphia, Pa.

The American Society of Zoologists.—December 30–January 1. *Eastern Branch:* President, Dr. Raymond Pearl, Maine Agricultural Experiment Station; secretary, Dr. Caswell Grave, The Johns Hopkins University, Baltimore, Md. *Central Branch*—December 29–January 1: president, Professor H. B. Ward, University of Nebraska; secretary, Professor W. C. Curtis, University of Missouri, Columbia, Mo.

The American Physiological Society.—December 29–31. President, Dr. S. J. Meltzer, Rockefeller Institute for Medical Research, New York City; secretary, Professor A. J. Carlson, University of Chicago, Chicago, Ill.

The Association of American Anatomists.—December 29–31. President, Professor Ross G. Harrison, Yale University; secretary, Professor G. Carl Huber, 1330 Hill Street, Ann Arbor, Mich.

The American Society of Biological Chemists.—December 29–31. President, Professor A. B. Macallum, University of Toronto; secretary, Professor Philip A. Shaffer, 1806 Locust St., St. Louis, Mo.

The Society for Pharmacology and Experimental Therapeutics.—December 30–31. President, Dr. Torald Sollmann, Western Reserve University Medical School, Cleveland, Ohio; secretary, Dr. John Auer, Rockefeller Institute for Medical Research, New York City.

NEW YORK CITY

The American Mathematical Society.—December 30–31. President, Professor E. B. Van Vleck, Uni-

versity of Wisconsin; secretary, Professor F. N. Cole, 501 West 116th Street, New York City. Chicago, December 26, 27, secretary of Chicago meeting, Professor H. E. Slaught, University of Chicago, Chicago, Ill.

The American Anthropological Association.—December 29–31. President, Professor Roland B. Dixon, Harvard University; secretary, Professor George Grant MacCurdy, Yale University, New Haven, Conn.

The American Folk-Lore Society.—December 31. President, John A. Lomax, University of Texas; secretary, Dr. Charles Peabody, 197 Brattle St., Cambridge, Mass.

PRINCETON

The Geological Society of America.—December 30–January 1. President, Professor Eugene A. Smith, University of Alabama; secretary, Dr. Edmund Otis Hovey, American Museum of Natural History, New York City.

The Association of American Geographers.—Probably meets at Princeton but official information has not been received.

The Paleontological Society.—December 31–January 1. President, Dr. Charles D. Walcott, Smithsonian Institution; secretary, Dr. R. S. Bassler, U. S. National Museum, Washington, D. C.

NEW HAVEN

The American Psychological Association.—December 30–January 1. President, Professor Howard C. Warren, Princeton University; secretary, W. Van Dyke Bingham, Dartmouth College, Hanover, N. H.

The American Philosophical Association.—December 29–31. President, Professor E. B. McGilvary, University of Wisconsin; secretary, Professor E. G. Spaulding, Princeton, N. J.

MINNEAPOLIS

The American Economic Association.—December 27–30. President, Professor David Kinley, University of Illinois; secretary, Professor T. N. Carver, Harvard University, Cambridge, Mass.

The American Sociological Society.—December 27–30. President, Professor Albion W. Small, University of Chicago; secretary, Scott E. W. Bedford, University of Chicago, Chicago, Ill.

WASHINGTON, D. C.

The American Association for Labor Legislation.— December 30–31. President, Professor W. W. Willoughby, Princeton University; secretary, Dr. John B. Andrews, 131 East 23d St., New York City.

MONTREAL

The Society of American Bacteriologists.—December 31–January 2. President, Professor C. E. A. Winslow, College of the City of New York; secretary, Dr. A. Parker Hitchens, Glenolden, Pa.

SOCIETIES AND ACADEMIES

THE BOTANICAL SOCIETY OF WASHINGTON

THE thirteenth annual meeting of the Botanical Society of Washington was held in the committee room of the Bureau of Plant Industry on October 17, 1913, at 1:30 P.M., with seventeen members present. The customary reports were presented and approved and the following officers elected for the ensuing year: *President,* C. L. Shear; *Vice-president,* A. S. Hitchcock; *Recording Secretary,* C. E. Chambliss; *Corresponding Secretary,* P. L. Ricker; *Treasurer,* H. H. Bartlett. Mr. F. L. Lewton was nominated as Vice-president from the society for the Washington Academy of Sciences.

The ninetieth regular meeting of the Botanical Society of Washington was held in the assembly hall of the Cosmos Club on Monday, October 6, 1913, at 8 P.M., with forty-two members and seventeen guests present, including the following distinguished European botanists: Frau Dr. Brockmann-Jerosch, Zürich; Dr. Edward Rübel, Zürich; Professor Carl Schröter, Zürich; Professor C. von Tubeuf, Münich.

The program consisted of brief informal remarks, as follows:

An address of welcome to the guests of the society, by President Stockberger.

"Citrus Plants of the World and their Importance and Use in Connection with Citrus Cultures and Citrus Breeding," by Mr. Walter T. Swingle.

"A Brief Summary of the Results of Twenty Years' Work with Mistletoe," by Professor C. von Tubeuf.

Professor Carl Schröter of Zürich translated Professor Tubeuf's address into English.

"Plant Introduction Work of the Bureau of Plant Industry," by Mr. David Fairchild.

"Impressions Received during the American International Phytogeographic Excursions," by Professor Carl Schröter.

"Nodule Production and Nitrogen Fixation by Plants other than Leguminosæ," by Dr. Carl Kellerman.

"The Chestnut Blight Disease," by Dr. Haven Metcalf.

"Photographs of Buckthorn Acacias," by Mr. W. E. Safford.

The ninety-first regular meeting of the Botanical Society of Washington was held in the assembly room of the Cosmos Club at 8 o'clock P.M., Tuesday, November 4, 1913, with forty-six members and five guests present.

Dr. Harry B. Humphreys and Messrs. G. C. Husmann and K. J. J. Lotsy were elected to membership.

The action of the retiring executive committee relative to giving a dinner in honor of the seventieth birthday of Dr. Edward L. Greene was called to the attention of the Society by the President, and a committee was appointed to arrange the details.

The following scientific program was presented:

Abbreviations used in the Citation of Botanical Literature: PROFESSOR A. S. HITCHCOCK.

Professor Hitchcock pointed out the different methods used for abbreviating citations, the extreme contraction on the one hand, such as "O B Z" (Oesterreichische Botanische Zeitschrift), and on the other the elaborate citations used by some authors in the *Pflanzenreich.* Abbreviations should be brief as possible consistent with clearness, but should follow a definite system. The speaker described the system followed in abbreviating citations used in the Contributions from the U. S. National Herbarium. The record of authorized abbreviations of authors and titles is indexed in a card catalogue. Authors consult this record when preparing manuscript for publication, thus aiding the editor in securing uniformity.

Non-parasitic Foliage Injury: MR. CARL P. HARTLEY.

Notes were given on the effects of drouth and storm on leaves of ornamental trees at Washington, D. C., for the past season. June and July were hot and dry, with but 35 per cent. of normal rainfall. Norway maple, especially in street planting, suffered most from drouth, the margins of leaves being killed; in the worst cases whole leaves except parts immediately adjoining the veins died. Most other trees, including *Acer rubrum,* escaped serious leaf injury. A northeast storm with hail and a 66-mile wind at the end of July injured many species, especially sugar maple and American basswood. The storm injury to maple resulted in the death of large parts of leaves at the margins and between the veins, without laceration or other external indication of mechanical injury. These storm-injured maple leaves could be distinguished from those hurt by drouth only by their limitation to parts of trees especially exposed to the northeast storm.

Pitfalls in Plant Pathology: DR. H. W. WOLLENWEBER (with lantern).

A revision of the hundreds of species of *Fusarium* in literature has led the writer to believe that the genus *Fusarium* contains only 30 to 50 different forms. To convince himself of this fact he intends to compare his pure culture strains with species of the important exsiccata collections of the old world.

A sharp criticism was given to mycologists who send unreliable specimens to the international "Pilzcentrale" in Amsterdam. Many errors are caused by the earlier opinion that *Fusaria,* as a rule, are adapted to one particular host.[1]

Sections of a Fossil Wood from Asphalt Lake near Los Angeles, Cal. (specimens): DR. ALBERT MANN.

Thin sections of the petrified wood were exhibited under a microscope which showed fungus hyphæ. Brief notes were given as to the apparent method of the growth of the fungus and the possible identification of the tree was discussed.

P. L. RICKER,
Corresponding Secretary

THE PHILOSOPHICAL SOCIETY, UNIVERSITY OF VIRGINIA, MATHEMATICAL AND SCIENTIFIC SECTION

THE first meeting of the session of 1913–14 of the Mathematical and Scientific Section was held October 20.

The following officers were elected to serve for the session: *Chairman,* Professor W. S. Rodman; *Secretary,* Professor L. G. Hoxton; *Publication Committee,* Professor W. H. Echols, Professor Thos. L. Watson, Professor Wm. A. Kepner.

Professor W. H. Echols read a paper "On the Expansion of a Function in Terms of Rational Functions."

Professor S. A. Mitchell presented a report of work done on an eclipse expedition to Spain.

L. G. HOXTON,
Secretary

UNIVERSITY OF VIRGINIA

[1] Lantern slides were shown to illustrate the difficulties the taxonomist meets, and these were explained and discussed.